BRITISH MEDICAL JOURNAL

LONDON SATURDAY APRIL 15 1939

THE LISTERIAN IDEA IN THE YEAR 1939*

BY

RENÉ LERICHE, M.D.Lyon, LL.D.Glas., Hon. F.R.C.S.Eng.

Professor of Surgery, University of Strasbourg

In reading Lister's papers, clear, precise, clean-cut, and bright, like the metal in our operating theatres, it has appeared to me that nowadays the fullness of the Listerian conception and its permanent application have not always been understood as they ought to be. Toying with the words "antisepsis" and "asepsis," many surgeons of the present day see the letter only and not the spirit of Lister's doctrine. They are inclined to the idea that Lister halted on the threshold of Pasteur's work. His ritual would then have been only one stage, soon exceeded. His method would then be only a momentary phase in a moving practice. And of Lister there would remain only a great name.

Such a conception would show a very imperfect knowledge of his work and thought. And indeed such an erroneous judgment has already been corrected in this place. The late Lord Moynihan put it thus:

> "We know that the old quarrel as to the relative merits of the antiseptic and the aseptic methods was senseless and jejune. For Lister was indubitably the parent of both. . . . Aseptic surgery is the wise practice of antiseptic surgery. There is a difference in detail, not in ideal or in fundamental truth."

It is true that boiling and dry heat have in great measure done away with carbolic acid, but these are only antiseptic means for the attainment of asepsis. The objective still remains the coagulation of the protein element of a microbe. Heat? Chemicals? In truth there is no essential difference between them. Heat is safer, easier to handle. We have adopted it. The principle remains the same—the achievement of asepsis by antisepsis. Again, it is necessary to note that the most rigid partisans of asepsis by heat remain faithful, in certain respects, often without knowing it, to the attainment of antisepsis by chemical means. They disinfect the skin with tincture of iodine, with alcohol, or with picric acid. They apply a chemical substance to appendix stumps and to the lines of intestinal section. Involuntary disciples of Lister, they are unable to escape from the dominance of his teaching.

Our interest then, in considering the present position of the Listerian idea, does not consist in the reopening of an old academic quarrel. The question is already settled. Our entire surgical practice of to-day is the direct outcome of Lister's research and teaching, in the light of the demonstrations of Pasteur. Lister was the first to understand, and in the beginning he fought alone, and alone withstood the blows directed against his teaching. Such glory as his is not to be shared.

Having said this, and once for all, our present interest in the subject is to see whether nowadays we do not require to get back to Lister's first idea, for it is just possible that, with all the improvements and apparent advances we have made in our methods of destroying germs, we may have too much overlooked one of the essential elements in the problem of sterilization.

Have the Germs in the Air a Pathogenic Quality?

In other words, is infection of wounds from the air, and by the germs contained in it, of any substantial importance? After his memorable conversation with Thomas Anderson, who told him of Pasteur's experiments, Lister conceived the idea that infection of wounds was conveyed by the air. He believed that it was the living particles in the air that were to be feared, and therefore to be destroyed. That is why he invented the carbolic acid spray. Nowadays we no longer trouble much about the air in our operating theatres. We mainly fear direct involuntary contagion, contact with everything that the surgeon employs and everything that is around him. Germs in suspension, floating in the air around, do not worry us. Are we right in so thinking? At first sight, everything seems to point towards an affirmative answer to the question. Under present surgical conditions infective complications of the suppurative type have become exceptions.

As a rule our operative wounds heal by first intention. They are neat, free from redness, and without obvious inflammation. And yet we know that they are not entirely free from germs. Our operation scars generally form without incident, but they are not aseptic, even though we speak as though they were. Bacteriological tests have long since proved this. For this very reason, as we are generally desirous of further improvement, recent years have seen a notable renewal of researches on the subject.

Petri dishes, brought into contact with our instruments, or even, as I have proved, with swabs used in the abdomen for packing the intestine out of the way, are always contaminated. Culture discloses the presence of colonies of staphylococci and of streptococci. The colonies are few in number if no one moves in the operating room. They

* The Lister Memorial Lecture (abridged) given before the Royal College of Surgeons of England, April 5, 1939.

are numerous if there is much movement, if there are many onlookers, if the latter are not duly masked, and if they approach close to the operative field. If Petri dishes are placed near the spectators the dishes all show numerous germ colonies. The experiments of Flügge and those of Trillat have shown us why.

On the other hand, germs are few in number if the operating theatre is kept absolutely closed, if the floor is constantly wet, and if the air is conditioned and filtered. This all means that the air of our operating theatres is usually heavily charged with germs when we are operating, and all the more heavily charged the more it is disturbed. The ordinary matter-of-fact surgeon, who is not readily disturbed and is easily satisfied, will say perhaps: "What, then, does it matter? Our patients do quite well, and nothing untoward happens." But it is indeed true that, although Petri dishes exposed in proximity to us furnish colonies of microbes, our operative wounds pursue an undisturbed and absolutely apyretic course. They heal by first intention, and without a doubt give us smooth linear scars.

But do none of them, in the course of a few months, widen out somewhat, become exuberant, perhaps a little blue under the influence of cold? Do not some of them become painful and more or less keloid? Do not some of them have a hard, sclerotic substratum? Are there none that give way—it may be months afterwards—causing, perhaps, eventration; although in the beginning they seemed quite firm and secure? Are there not a number of so-called aseptic operations that result in peritoneal adhesions, even though no visceral cavity has been opened and there is no question of endogenous infection? And when some operative procedure has apparently cured an old septic lesion of only moderate virulence, only to be followed by a fresh and sudden infective attack, are we not entitled to suppose that there has been a fresh infection superimposed on it? Finally, do not vascular complications—phlebitis and embolism—still occur, even although otherwise the post-operative course has been uninterruptedly satisfactory, and the process of healing apparently normal?

In view of all this, it seems justifiable to raise once again the question as to what deleterious influence may be exerted by infection having its origin in the air of our operating theatres. Though not actually pyogenic, may it not perhaps be capable of proteolysis, of modifying local protein structure, of slowly dissolving new-formed elastic architecture, of perverting local tissue metabolism? To put it another way, may not this masked or imperfect infection, incapable of clinical detection, and which we regard as actually of little account, have effects on the tissues which hitherto we had not ascribed to the real cause?

One must not forget that the word "infection" has come nowadays to have a much wider meaning in microbiology than that which we surgeons give it—especially since the peculiar effects of ultraviruses and of certain protein molecules have become known. If this is so, might we not be wise to return to the Listerian idea, and to endeavour, from now on, to eliminate even the least possibility of air infection?

Role of the Constitutional Factor in the Post-operative Development of a Wound

Personally I am inclined to think that the quality of our operation scars, their aesthetic value, their firmness, and the future of their development depend, above all, on the constitution—local and general—of the patient's tissues. Under similar operative conditions, every patient heals in his own particular way, because each has his own tissue values, which may be regarded as a reflex of his hormonal personality.

Without any doubt, attention to detail and the development of the most perfect technique have, up to a point, a definite influence on post-operative healing; but no given surgeon obtains, always and at all times, the satisfactory scars he may deserve. Some of our patients have linear, soft, and satisfactory scars, while the scars of others are coarse and exuberant; and we are powerless to determine the issue.

Every repair depends very much on the vitality of the tissues—a factor which sometimes, of itself, defeats the best of technique. Our scars are composed mainly of connective tissues, and connective-tissue vitality is regulated normally by circulatory and hormonal phenomena that have a different tonality for everyone. It is true that there is only one general plan of wound evolution, but, from one man to another, what variations there are! During the process of healing, these circulatory and hormonal values come into play, each in its own way, and always with its individual characteristics. Quick-acting and well-balanced vasomotor reactions generally assure the development of perfect scars. Sluggish reactions—as when there is a degree of stasis—lead to hypertrophic scars. The subjects of certain hypophyseal conditions have elastic tissues of poor quality, while people with excess of parathyroid have satisfactory collagen. Many other factors, still unsuspected, doubtless influence tissue repair. One day, perhaps, we shall have "hormonally directed healing," and then our patients will be spared many discomforts.

These reflections indicate to you that I am personally inclined to turn in quite a different direction from the one I discussed previously. But I readily admit that even these reflections are not sufficient to justify the setting aside of the other, the microbic, hypothesis. It is first of all necessary that we should study for a little the means that have been suggested to protect our patients against air infection. Two of these may be considered. They are: sterilization of the air by physical agents, and some arrangement such as the complete outfit devised by Gudin, which has for its purpose the attainment of what he has called "integral asepsis."

Are Physical Agents Antiseptic?

Is it the case that certain forms of irradiation are able to exert an effective action on the microbic flora of the air in the vicinity of a wound? Some writers have affirmed that this is so as regards *ultra-violet rays*, the most precise and consistent observations on the subject being those of Hart and of Santy.

Hart employs special lamps that irradiate the wound throughout the whole operation. He has found that, exposed in this way to the rays, Petri dishes containing staphylococcus cultures are sterilized in a few minutes. Sterile Petri dishes placed near the operative field remain sterile if irradiation is carried out, while in the absence of irradiation they become contaminated.

Santy's operating theatre is so arranged that the air is made to pass in front of a powerful generator of ultra-violet rays, and is then sent back through a charcoal filter. He has found that, under such conditions, Petri dishes which would ordinarily show thirty to fifty colonies at the end of an operation show hardly any (perhaps four to five colonies per dish), even if there are many onlookers. It seems that, with a few improvements, a complete

sterilization might be reached. We may have here the promising beginnings of further great surgical progress, and we shall probably be able very soon to reach a new stage in aseptic technique.

Are ultra-violet rays the only ones to act in this way? For the present, apparently one must answer the question in the affirmative. *Infra-red rays* do not seem to have similar germicidal properties. At the instigation of Paschoud I have during the last two years operated quite a lot under infra-red rays—over a thousand times—and it has not appeared to me that, clinically, infective conditions calling for operative intervention have behaved in any way differently, according as they were or were not irradiated. And in aseptic surgery I have met with cases of phlebitis and embolism in patients who had been operated on under adequate infra-red irradiation.

I am therefore convinced that infra-red irradiation makes no essential change in the ordinary evolution of healing of surgical wounds. This does not necessarily imply that exposure to infra-red rays makes no change at all in post-operative development. Patients certainly suffer less, and seem to be more comfortable after their operation. This is probably to be explained by the fact that infra-red rays cause hyperaemia, and this hyperaemia modifies the quality of the post-operative sympathetic reactions, though it appears to have no real action on the infection itself. I have placed Petri dishes actually in the wound whilst operating under the infra-red rays, and the usual microbic developments have taken place.

We have to be content, therefore, with the ultra-violet rays, which, unfortunately, have no analgesic effects. A combination of infra-red and ultra-violet rays might perhaps be efficacious; but, on the other hand, it might have some drawbacks, both for our patients and for ourselves. For this reason it appears necessary for us to explore other possible lines of action.

Gudin's Integral Asepsis

Is it not possible to operate, as Pasteur would have wished, in a completely sterile atmosphere, in a theatre where everything is sterile, into which no one should enter unless clad from head to foot in sterile garments; a theatre where anything could be touched without the slightest risk, and anything could be moved about without danger because air, walls, furniture, floor—in fact, everything—would be germ-free? That is the question that Maurice Gudin of Rio de Janeiro put to himself a few years ago. Long before his time the air in operating theatres had quite often been sterilized by steam or by antiseptic spraying. I remember visiting, thirty-three years ago, an organization of this type in Kraske's clinic. But, as soon as operator and assistants entered the theatre, germs entered with them, and absolute asepsis was little more than an empty word. This attempted effort at improvement came to be regarded as an unnecessary complication, and the idea was soon abandoned.

Gudin's objective is altogether different. To him the credit is due of being the first to realize the necessity for a really complete outfit. For this purpose he established a group of sterilized rooms, so disposed that patient, surgeon, and assistants shall enter a completely sterile theatre only when they themselves are identically aseptic, dressed from head to foot in sterile garments, the operators having only a glass-protected opening for their eyes and the patient an opening for the operative field, suitably antisepticized. In order to obtain complete sterilization of the different rooms and their interior fittings Gudin uses, by means of a special apparatus, formaldehyde vapour, which finds its way into every corner, and which is later neutralized by ammonia. Bacteriological tests have shown him that under such conditions complete sterilization can be achieved. Door knobs may be handled, swabs may be taken from shelves, without there ever being any possibility of contamination: asepsis is "integral."

I have myself operated in Gudin's sterile block, and in spite of the smallness of the operating theatre, with its roof of glass enabling the assistants to overlook the operating table, I was completely at ease and operated without tiring. The air is conditioned. The temperature is pleasant. Silence is complete. And I had really a feeling of great security.

Here you have, then, actually in being, and after the lapse of three-quarters of a century, a revival, in greatly improved form, of the Listerian idea of sterilizing the air by chemical means; a revival in strict conformity with the ideal of Pasteur, with whose views on such a matter Lister was always in agreement. Others besides Gudin, working on similar lines, have achieved a form of integral asepsis. Trenel filters the air by means of cotton and oil in a closed circuit, and then regulates its temperature. Masmonteil uses superheated sterile steam, which is extracted when it has produced the desired effects in precipitating the microbes. Both Trenel and Masmonteil, of course, have an arrangement of successive rooms, in order to avoid the bringing in of germs by operator, assistants, or by the patient himself. The result in the way of sterilization seems to be quite effective. Masmonteil states that bacteriological tests show that the air is almost sterile, that the walls are sterile in 85 per cent. of cases, and the floor in 75 per cent. Here also, then, we appear to have an acceptable solution of the problem under discussion. Are the practical results, however, worth the effort of rebuilding our operating theatres, and the trouble of completely altering our ways of working?

Gudin compares the usual healing by first intention, under conditions not entirely germ-free, with healing that is really aseptic, which he terms Pasteurian, and which he believes to have positive advantages. Is he right? In theory there can be little doubt that he is. No one could reasonably say the contrary. In actual practice it is clearly possible, though perhaps not yet completely proved. Only a long comparative experience can decide, because it is a question of minute differences, and shades of difference, very difficult to appreciate. It would, however, be a mistake for us to abandon interest in the subject.

It may be that in integral asepsis we have a condition of progress—a progress of which we do not yet know the full significance. I have given full weight to all the theoretical objections that might be made to Gudin's conception—difficulty, inconvenience, relative uselessness, expense. None of these is of determining value. We must never forget that, even in Lister's time, no one but Lister at first saw any reason for changing the traditional practice of surgery. Everything was as good as it need be! All that was done was perfect. And yet what terrible failures! And, in more recent years, have we not seen numerous surgeons who strenuously opposed the use of masks or of gloves? Even a surgeon is a creature of habit.

For my part, I believe that some international organization might well undertake a fresh study of the subject of integral asepsis, a subject which is of interest to the whole world. Such an organization might well supply the necessary facilities of study to a group of investigators noted for their powers of research and of discrimination. For

the present, only one definite statement can be made in this connexion—namely, that the Listerian idea still dominates the evolution of surgical thought, and that it has not yet ceased to give promise of fruit.

Treatment of Wounds

The Listerian conception, however, was not concerned merely with operative antisepsis. Its primary purpose was to achieve the disinfection of wounds and their sterilization by chemical means. Where do we stand to-day with regard to this very problem?

Up to the year 1914 we had very little exact knowledge of the changes involved in the healing of wounds, or of how wounds might be disinfected. Four years of war surgery, however, brought under our eyes all at once an enormous mass of experimental data, which we had the opportunity of examining from start to finish. No laboratory worker had ever before dreamed of experimentation on so vast a scale. One had only to observe. Each day brought us its fresh batch of completed experiments. The inevitable result of the infection was beyond dispute. The conditions were obvious. Definite conclusions could really be arrived at, conclusions free from any ambiguity, provided that due care were taken to employ really comparable series of cases. In my view, in spite of the successes of many chemical substances—of bipp, of eusol, of the hypochlorites, of the various aniline dyes and essential oils—and in spite of the magnificent results of the Carrel treatment, the conclusion is inevitable that the large-scale experiment of 1914 to 1918 established the failure, relative but none the less unmistakable, of antiseptics. It showed that what determined the fate of a wound was, above all, the vital quality of the tissues exposed to infection, and not the use of this or that chemical product. Any wound that was clean-cut and free from contusion, if left freely exposed to the air, healed of itself, without any antiseptic aid.

On the other hand, an irregular, ragged wound with separated edges and necrotic walls suppurated for a long time, even when treated by appropriate antiseptics; while if it were carefully excised it remained sterile and could be sutured. The essential point was that the excision should be carried through healthy tissues. Direct surgical action, then, superseded chemotherapy. Without such surgical intervention nothing was of any use. With it, success was generally assured.

Certain it is that about the year 1917 we realized that, without the slightest chemical treatment, and without risk, we might proceed to suture a well-excised wound even if it still harboured staphylococci or perfringens bacilli, provided that the neighbouring tissues were of satisfactory vitality. Note that these observations are quite in line with conclusions drawn from bacteriological studies carried out on the operative wounds of present-day aseptic surgery. Here, too, infective complications do not develop, in spite of a certain degree of contamination, if haemostasis is efficient and the tissues are not traumatized.

What I have just said applies particularly to fresh wounds treated within a few hours of their reception. May the same be said of wounds clinically infected? *A priori* one might say of these that certain antiseptics have proved their efficiency in such cases. But, even so, their effects require analysis.

Nowadays we know well enough, after seeing so many wounds suppurate despite all our efforts, that antiseptics are only successful in disinfecting wounds that are laid completely open. And we know that, whenever there are ragged edges, recesses, lacerated necrotic tags of tissue, blind tunnels, or dead spaces, even the best antiseptics invariably fail. And the best antiseptics are not those which, *in vitro*, have the greatest germicidal power, but those that have a powerful proteolytic action, such as Dakin's fluid, which rapidly destroys dead or dying tissues, and acts, therefore, like a chemical bistoury. It would probably be true to say that the process by which the germs disappear is of little moment so long as they do disappear. And indeed such a proposition would not be actually incorrect. Nevertheless, if we are to make real progress in our art we must understand things aright and not be satisfied with mere words. We must try to formulate the essential conditions of disinfection of infected wounds. Only then shall we be able, on the basis of actual knowledge, to see and understand the laws governing antiseptic action.

The Essential Conditions of Disinfection

The first condition has reference to the wound itself—its characters and architecture. The only wounds that have the capability of spontaneous disinfection or that are readily disinfected by antiseptics are those that are freely exposed to the air in every part. A deep wound cannot be effectively disinfected by the use of a chemical substance and a drainage-tube. Neither of these can achieve the simple beneficial effect of a clean cut with scissors. The most valuable of all antiseptic agents is the bistoury, by the use of which the whole of the injured area can be given free exposure; and Carrel's method only achieved success when it came to be realized that an initial and complete surgical preparation was a necessary preliminary to its application.

Even now many surgeons fail to realize the practical importance of the systematic opening up of wounds, and in peace-time, even as in war-time, the majority of failures registered in the treatment of infected wounds are due to surgeons failing to respect this definite law.

The second condition of disinfection is the complete and prolonged immobilization of the wound and part. In order to understand fully the importance of this condition it is necessary to specify exactly what happens in the case of a wound imperfectly immobilized—a wound not properly put at rest. We all know that the dressing of an infected wound, especially if it is situated near a bone or joint, is generally followed by a brisk rise of temperature. What is less well known is that prior to the dressing a blood culture may be sterile, and yet two hours after the dressing it is no longer so—the culture is positive. Movement, therefore, in the case of certain infected wounds, gives rise to a transient septicaemia. This accepted fact of clinical experience adequately explains the organic disequilibrium that follows certain dressings.

That was why Ollier, reasoning from simple facts of observation such as these, long ago made a systematic practice in all his bone surgery of changing dressings as seldom as possible. He dressed his resections only after a long interval—a month, six weeks, or even longer, after the operation. This practice has indeed remained traditional at Lyons for quite three-quarters of a century, and has hardly been changed at all. More recently, in America, the advantages of immobilization have been rediscovered and proclaimed by Orr, and it cannot be disputed that these advantages are generally very remarkable.

Even serious and acute infections of bones or joints may quiet down within a few days if the affected part or limb is adequately immobilized in plaster—and this in spite of the unpleasant odour which the dressing soon

acquires. Within a few days the temperature may come down to normal and the general conditions of the patient alter for the better. His appetite returns and his whole appearance is different. After several weeks, it may be, when the dressing is changed for the first time, the wound is found covered with abundant fleshy granulations, well vascularized, of a good colour and remarkably healthy appearance. The erythematous skin around the wound is covered by a thick, sticky, wax-like exudate. Bacteriological examination of this exudate shows an abundance of micro-organisms, just as may be found in the dressing material and bandages. On the other hand, however, only a few germs are to be found on the surface of the wound itself—not more than two or three perhaps in the microscopical field. And in the depths of the wound the tissues are germ-free. I have proved this fact many times.

What exactly has happened? One cannot yet say. The micro-organisms have not merely died out, since outside the actual wound they are active and very numerous. It would appear as if they had been simply washed out of the wound by the exudation of lymph, the direct result of the local congestion of the deeper tissues whose normal static equilibrium, on which the humoral balance depends, is defective or entirely absent. The same phenomenon can be seen after exposure of wounds to the sun, even as it can be demonstrated in cases treated by hypertonic solutions, as advocated by Sir Almroth Wright. In any case, there is the fact, and there are the results. We know that immobilization is perhaps the most valuable of all our therapeutic means of combating wound infection.

Listerization by the Intra-arterial Route

When the two conditions of wound disinfection which I have just mentioned have been fulfilled, then, but only then, may chemical antiseptics be used in addition. Their employment is not a mere prelude to or accessory in treatment of questionable value. But what antiseptics ought one to employ? Surgeons of the present day no longer make use of carbolic acid, but they are not forgetful of what surgery owes to it. In an Address on Lister, delivered in 1927 during the Lister Centenary Celebrations, my distinguished friend Archibald Young recorded and contrasted in a very striking way the pre-Listerian and post-Listerian statistics of the Glasgow Royal Infirmary. They speak for themselves.

Ought we to remain faithful still to carbolic acid, as Lucas-Championnière in Paris, and my master, Poncet, in Lyons—who was a disciple of Lister from the very beginning—remained to the end of their lives? Or should the vogue of carbolic acid be accepted as a thing of the past? We no longer know how to use it. In its stead we make use of the lytic action of the hypochlorites and we have recourse to germicides of the type of mercurochrome. Given the conditions specified above, these are certainly of great service in the treatment of wounds, but even they have only a limited scope. Their action in reality is only superficial. Infection in the deeper tissues is unaffected by them. Direct contact is required between the germicidal agent and the micro-organisms if a successful result is to be achieved. The germicide very quickly decomposes, or tissue debris shuts it off from close contact with the germs it ought to encounter. Hence the necessity, as Carrel pointed out, for a constantly renewed supply of germicide.

That is one reason why, in ordinary daily practice, chemical disinfection is difficult to use, especially in complicated cases, and why it meets with many failures. It is not sufficient merely to register these failures. They ought to have the effect of urging us to give careful consideration to the evidences that come before us from day to day. Our observations teach us, surely, that real disinfection acts from within outwards, beginning in the depths, and later reaching the surface. Why, then, should we shut our eyes to the evidence that we have, especially since we have at our disposal nowadays means of attacking the germs even in the tissue depths?

And in speaking of this, I am not thinking of the intravenous route—wasteful as it is—of which one wonders if it can possibly be efficacious in a peripheral lesion. Nor am I thinking even of the intracardiac route, which I have employed in certain cases of septicaemia. I have in mind, rather, the intra-arterial route of Goyanes, which has been taken up again by Reynaldo dos Santos, and regarding which dos Santos has recorded some striking results. Dos Santos, over ten years ago, demonstrated that certain serious infective conditions could sometimes be arrested by injecting through the skin, and directly into the main artery of the affected region, either gonacrin or mercurochrome. His collaborator, Cunha Lamas, published statistics of 120 cases of different types of infection treated in this way by intra-arterial Listerization.

At the Strasbourg Clinic, since the year 1929, we have been applying the intra-arterial method in quite a considerable number of conditions. We have stopped counting up the successes it has given us, in suppurative arthritis of all sorts, in early cases of osteomyelitis, in lymphangitis, and in the pregangrenous infective lesions of arteritis.

Only the other day I had under treatment a young man who was suffering from a suppurative arthritis of the elbow following upon injury. He had had a temperature of 104° F. for three days. He received an injection of 10 c.cm. of a 1 per cent. aqueous solution of mercurochrome. This was given into the axillary artery, and within a few hours his temperature fell to 99.5° F. and remained down.

Our experience of ten years, then, would seem to justify us in claiming that disinfection by the introduction of antiseptics by the arterial channel is destined to become the method of choice. Further, in view of the continuing progress of chemotherapy, and the great hopes we have of the prontosil group of drugs, it is probable that very soon we may be able to arrest, without much sacrifice of tissue, the progress of many infective lesions that hitherto have demanded serious mutilation.

Obviously the method calls for technical and manual dexterity. Its employment requires judgment. It is not a panacea. It has its limitations. Infective conditions produce anatomical disturbances which may render its action incomplete or useless. It therefore has its failures. Yet, with a due sense of the importance of what I am saying, I have no hesitation in claiming that Listerization by the intra-arterial route is a great step forward, and such a forward step may lead to further advance.

Intra-arterial Novocainization

Observation of patients treated in recent years by novocainization has enabled us to analyse more closely the physiological mechanism which infection brings into operation in the tissues. A considerable disturbance of the local vegetative equilibrium is brought about. It follows that, if novocain is infiltrated into and about the region involved in sufficient quantity to block vasomotor innervation, an incipient infective process may be aborted. In the same way novocain infiltration of sympathetic ganglia may at once arrest the further development of a local or latent infection. In view of observations such as these at the Strasbourg Clinic, we were led ultimately to inject novocain into the main artery of a limb in which

an acute infective process was beginning. The infection subsided.

It would seem, then, quite probable that, in addition to intra-arterial Listerization—frankly antiseptic in its purpose, and aiming at the direct destruction of germs—we have here a possible method of getting at the very mechanism of the inflammatory process and of averting by physiological means the deleterious consequences of an infective inoculation. We are continuing our researches along this line, and have already obtained some unquestionably satisfactory results in salpingitis and in certain pelvic infections, as already recorded by Daniel.

Here again, then, we have ground for substantial hope. The main problem has assumed a new phase. Up to the present all efforts to treat infective processes have been anti-microbic. These later efforts are directed to the patient, the microbe's host. Just as it takes two to make a quarrel, two elements are necessary for the development of a disease—the host and the infecting organism. It can hardly be disputed that much is to be gained by looking after the one as well as the other. This is only a further demonstration that the Listerian conception is still capable of bearing fruit in our minds.

The Lesson of Lister

Such fruitfulness does not surprise us. Ever since I received the honour of being asked to address you I have been trying to penetrate the official and almost legendary figure of Lord Lister and appreciate the real man. Any man of outstanding personality is apt very soon to be depicted in legend and history on a somewhat artificial pattern. With the passage of time the hero becomes more and more an almost abstract figure, often quite different from what he was in life.

Through the pages of many addresses and panegyrics I have sought to find out what, after all, was the inner rhythm or essence of Lister's mind. Having analysed him to the best of my ability, I desired to reconstruct for myself a picture of the man. It is clear that that is not an easy thing to do. Even in the case of living people we never see more than the shadowy outline of their personality, and we see it only through ourselves. On the other hand, the work that a man has done may well reflect the man himself, and we are often able to construct for ourselves a picture of the man through the lessons he has taught us—and, in so doing, not go very far astray.

We have learned from Lister the supreme virtue of intuition, the strength of reason based on experimental research, the creative power of persevering effort, of disinterested effort that is never satisfied but always strives to do better still. And so Lister appears to me, above all, a man of unusual type, to whom truth was directly intuitive, a man of complete honesty of mind and thought, and deeply sensitive to everything human. I picture him to myself a more or less solitary figure, working at his researches alone, his entire life centred on his purpose, ever master of himself, and determined yet restrained in all his ceaseless experimentation.

His genius was not the kind that, in tumultuous fashion, carries everything before it. It was the sort which achieves victory slowly through the radiant glory of truth itself. Lister's work was securely based upon the solid rock, and if ever work was beneficial to mankind his surely was. Gratefully, therefore, and with all respect, I salute Lister, the man, and to you, gentlemen, I tender my sincere thanks for having favoured me with the opportunity of doing so in this place.

OBSERVATIONS ON THE SURGICAL TREATMENT OF THYROTOXICOSIS

BY

J. W. LINNELL, M.D., M.R.C.P.

Consulting Physician to the L.C.C. Thyroid Clinic, New End; Physician, Metropolitan Hospital

AND

GEOFFREY KEYNES, M.D., F.R.C.S.

Consulting Surgeon to the L.C.C. Thyroid Clinic, New End; Assistant Surgeon, St. Bartholomew's Hospital

It is difficult for those who are daily observing the condition known as toxic goitre in its many clinical varieties to obtain a true view of its relative frequency in the population. A large proportion of these patients naturally gravitate to goitre clinics, and, in addition, the physician or surgeon who is fully "goitre-conscious"—that is, whose eye is fully trained to observe the less obvious signs of toxicity—is continually encountering the disease in unexpected places. Sometimes the signs have not been noticed by the medical men who are attending patients for apparently unrelated conditions. It would seem, indeed, that the disease is commoner than is even now generally realized, and for this reason we add the following observations to the already large volume of literature on the subject.

The Question of Operation

Some physicians are still opposed—almost, it seems, on principle—to operating for toxic goitre. Many spontaneous cures, it is said, have been seen. It is true that a few patients, usually among the younger ones, will permanently lose the toxic symptoms within a few months of their onset without treatment of any kind, or with rest and sedatives only. A few more, able and willing to submit to a semi-invalid existence for a long period, perhaps for years, may also be cured without operation. For this reason we often advise a period of observation for young patients in whom the symptoms have only recently appeared. Unfortunately, however, it is impossible to know whether any given patient is potentially a member of the spontaneously curable class, so that it is usually unwise to prolong the observation period beyond a few months unless the patient is obviously improving. Among the older patients the nodular form of goitre is common, and it is quite certain that when it becomes toxic it is virtually never cured by medical treatment alone. Even among the apparent "cures" of toxic colloid goitres it is found that in the great majority toxic symptoms recur after varying periods. Thus the patients eventually come to surgical treatment, having forfeited normal activity for several years of their lives in vain. In fact, many of the so-called cures are more apparent than real. Every patient with thyrotoxicosis shows alternating phases of exacerbations and remissions, and in the milder examples a remission may easily be mistaken for a "cure." In addition, a thyroid gland which has been apparently quiescent for years may be reactivated by conditions such as pregnancy, miscarriage, lactation, the menopause, an acute infection (for example, tonsillitis), an anxiety state, and mental or emotional shock. An anxiety state may, indeed, be induced merely by the presence of the goitre itself. Often quite small goitres make their presence felt by a slight sensation of pressure in the throat. The patient is thus constantly led to feel it and examine it in the mirror, until she has convinced herself that it is enlarging and may be harming her in various imaginary

ways. She fears also the development of toxic signs, of which she already knows, such as exophthalmos. It is even conceivable that in this way she may literally scare herself into a toxic state.

Subtotal Thyroidectomy

We therefore find ourselves in full agreement with Joll (1932), who stated that subtotal thyroidectomy is "the method which most rapidly, most certainly, and most safely restores the patient to a state of economic efficiency and involves least likelihood of relapse." Until comparatively recent times distrust of surgical treatment was natural in view of the high mortality which accompanied the operation, but this attitude is no longer justified by the facts. During the last five years one of us has operated on 330 patients with toxic goitre at the L.C.C. Thyroid Clinic at New End Hospital with three post-operative deaths—that is, less than 1 per cent. In a consecutive series of 1,000 patients operated upon by him at New End, St. Bartholomew's Hospital, and in private practice, only thirteen have died (1.3 per cent.). In these series there has been no selection of patients whatever. Practically every patient, however ill, has been accepted, and many of them, who have started as apparently almost hopeless risks, have been ultimately restored to an active and useful life.

The Use of Iodine

There is still a widespread belief that with the frequent administration of iodine the need for surgical treatment has been lessened. This view is erroneous, and we cannot too strongly emphasize the fact that iodine is never curative. It is true that, for reasons which are not yet fully understood, iodine tends to abate the high basal metabolic rate, the tachycardia, and most of the other symptoms so often present in thyrotoxicosis, but its effect is only temporary. The maximum effect is usually obtained within a fortnight, so that it may be of the greatest value during the pre-operative period, and it has in this way been an important factor in the reduction of post-operative mortality. It is also sometimes of value when given for a short time after operation, and its use should in our opinion be restricted to these periods. The practice of giving iodine continuously for many months in the belief that it cures is deplorable, for this may result in postponement of operation until heart complications, extreme emaciation, or a psychosis supervene. It should, in addition, not be forgotten that thyrotoxicosis has seemed to be induced in some non-toxic goitres by prolonged iodine medication. In pre-operative treatment iodine has come to occupy a much less important position than it did a few years ago, and it may even be maintained that iodine has been more of a curse than a blessing in the treatment of toxic goitre.

X-ray Therapy

X-ray therapy as an alternative to surgical treatment has in our view a very limited field of usefulness. It is of no value whatever in the large class of nodular goitres with toxic symptoms, and even in diffuse hyperplasia it is most uncertain in its effects. Relapse is very apt to occur, and we have seen many patients with established auricular fibrillation who had previously been "cured" by *x* rays. It appears, however, to be occasionally successful in very young people with diffuse hyperplasia of the gland, and it has sometimes been of great value in the rare event of relapse due to regeneration of thyroid tissue after subtotal removal. Speaking on this subject at the B.M.A. Annual Meeting in Melbourne, Sir Thomas Dunhill (1935) said that he had seen benefit result from irradiation in six out of thirty patients in the early stages of the disease, especially in children, though of 140 adults so treated no fewer than 118 had established auricular fibrillation following "cure" by this means.

Cardiac Complications

The majority of toxic patients who are not treated surgically die from cardiac complications after a long period of invalidism and misery due to progressive congestive failure of the heart. If it were generally realized that thyrotoxicosis is, after acute rheumatism, by far the most frequent cause of auricular fibrillation, it would be admitted that, for this reason alone, the risks associated with prolonged medical treatment are much greater than those of operation, now that operative mortality can be reduced to vanishing-point. Toxic goitre can also cause auricular flutter, both paroxysmal and established, and paroxysmal tachycardia. A remarkable fact, which we believe is not known even to most thyroid experts, is that between attacks of paroxysmal auricular fibrillation due to toxic goitre the heart rate may be normal or even subnormal.

In the same connexion there are other important points to be remembered. A goitre, patently toxic at one time, may seem to "burn itself out" in the course of years. The classic signs and symptoms of thyrotoxicosis may almost disappear, the gland decrease greatly in size, and the patient be apparently restored to health. Yet without any obvious recrudescence of activity in the gland cardiac complications may ultimately develop. Again, a goitre may never at any time produce symptoms severe enough to attract the notice of the patient or her doctor, and yet in the end cardiac complications may appear. It is well known that many adolescent goitres persist and become toxic in later years. It has even seemed to us true to say, as the outcome of extensive experience, that very few long-standing goitres in adult life are absolutely non-toxic. Whether or not this statement be an exaggeration, it is quite certain that there are a distressingly large number of victims of such cardiac conditions as we have mentioned in whom their association with a seemingly harmless goitre has never been suspected. Moreover, the goitre is sometimes so small that a careful search is needed to detect it, or its presence may only be revealed by an *x*-ray screen examination or film of the mediastinum. At the present time a wise cardiologist, in examining a patient with auricular fibrillation or flutter, established or paroxysmal, or even with a simple paroxysmal tachycardia, proceeds almost automatically to look for a goitre if the condition cannot be fully explained on other grounds. If one is discovered, the heart condition can usually be attributed to its presence. In addition we have several times met with patients suffering from hearts damaged by acute rheumatism who are further handicapped by toxic goitre. Even these are capable of a considerable degree of recovery when the goitre has been removed.

The Clinical Picture in Toxic Goitre

We have found that it is not always possible to correlate the cardiac or other complications of toxic goitre with a characteristic clinical picture. Thus it is undoubtedly possible for a considerable degree of toxicity to coexist with a plump habit of body, or even with a gain in weight, although the patient is not under treatment. Again, the greater our experience becomes the less weight do we attach to the estimation of the basal metabolic rate. It is, for instance, not infrequent for auricular fibrillation to develop as a result of toxic goitre in a

patient whose basal metabolic rate is within the limits of normal or is even subnormal. We have long recognized that all estimations of the basal metabolic rate must be accepted with reserve, the margin of experimental error and variations in the same patient on different occasions being wide. Clinical experience has always been a better guide, and justification for sometimes ignoring even a normal or subnormal figure with auricular fibrillation has been given by rapid restoration of the heart to a normal rhythm after removal of the goitre.

The fact that most goitres become to some degree toxic in the long run and the frequency of cardiac complications have encouraged us to believe that it is a sound practice to remove nearly all goitres in adults without undue delay, whether considered toxic or not. The argument for the removal of non-toxic goitres is further strengthened by the fact that they will so often compress or distort the trachea as they grow and that a small proportion of them will become malignant. Malignancy we find to be not so fatal a condition as it was once thought to be, though it is sufficiently serious.

Points in Operative Treatment

It is not our purpose to deal in this paper with operative technicalities. We must, however, emphasize that a very low post-operative mortality rate such as we have described can only be achieved by team-work, in which surgeon, physician, anaesthetist, and nursing staff all play their parts. Both preparation and after-treatment are matters of supreme importance, and since the patients so often present difficult cardiac problems, the services of a physician who is an experienced cardiologist should be obtainable if possible. It is often for him to decide on the best moment for operation, which should not be undertaken even by a first-class general surgeon unless he has already assisted, or at least closely watched, an expert at work on a series of patients. No two goitres are alike, and it is often the simplest-looking thyroidectomy that proves to cause the greatest difficulty for the surgeon and danger for the patient. Though technical skill on the part of the surgeon is most necessary, the patient's safety is as much dependent on good judgment as on manual dexterity. No decision in the whole realm of surgery is more dependent on experience than that concerning how much of the gland can be safely removed on one occasion, since the patient's reaction depends on the amount of disturbance rather than on how much active gland remains. It is our custom for physician and surgeon in conference to make a provisional estimate before operation, which is planned in such a way that the decision may be changed at any moment during its course. The complete operation can generally be performed safely in one stage, but sometimes only one lobe is removed at the first operation and the other is dealt with two or three weeks later. Sometimes the superior thyroid arteries are first ligatured under local anaesthesia, and the necessary amount of gland removed in one or perhaps two subsequent operations. Occasionally the steps must be even more gradual than this. It is somewhat surprising that cause for alarm has not been found so much among the older patients with auricular fibrillation, even when previously associated with congestive heart failure, as among the intensely nervous girls, who have remained restless and frightened in spite of careful preparation.

Conclusion

A close follow-up of patients who have been subjected to subtotal thyroidectomy has convinced us that it is one of the most satisfactory operations in surgery. It is true that few sufferers from thyrotoxicosis are *completely* cured by the operation, for imbalance of other ductless glands almost certainly coexists. Yet the great majority of patients can resume approximately normal lives and feel a sense of well-being which most of them have not known for years. Those who have complained of palpitations (with regular heart rhythm) tend to lose them. Nearly all with paroxysmal auricular fibrillation remain free from attacks, and a majority of those with established auricular fibrillation regain normal cardiac rhythm, with, or often without, the use of quinidine, and this as a rule appears to be permanent. In those who fail to regain normal rhythm the ventricular rate is much more easily controlled by digitalis than before. Nearly all the patients become free from the uncomfortable sense of heat, their nervousness and irritability diminish, and a normal weight is regained.

The results of operation have been on the whole so good that sometimes too much is expected of it. Many patients suffering from thyrotoxicosis have a fundamentally unstable nervous system and mentality, and naturally this cannot be changed by thyroidectomy.

In this connexion we have noticed that men tend to be more nearly completely restored to normal than women, perhaps because their psychological make-up is more stable and because they are not subject to the stresses of a menopause.

REFERENCES

Dunhill, Sir Thomas (1935). *British Medical Journal*, **2**, 800.
Joll, C. A. (1932). *Diseases of the Thyroid Gland*, London.

BIOLOGICAL PREGNANCY DIAGNOSIS TESTS

A COMPARISON OF THE RABBIT, THE MOUSE, AND THE "CLAWED TOAD" (XENOPUS LAEVIS) AS THE EXPERIMENTAL ANIMAL

BY

F. A. E. CREW, M.D., D.Sc., F.R.S.

(*From the Pregnancy Diagnosis Laboratory, Institute of Animal Genetics, University of Edinburgh*)

Impressed by the reports of Bellerby (1934) and of Shapiro and Zwarenstein (1934) concerning the usefulness of the "clawed toad" of South Africa (*Xenopus laevis* Daud.) as an experimental animal in biological tests for the diagnosis of pregnancy, I imported 1,500 well-grown females in 1937, being greatly aided in this matter by Dr. Zwarenstein, who, in Capetown, made the appropriate arrangements for their safe dispatch. Since then my colleagues and I have slowly been acquiring a knowledge of these animals and their needs when kept under artificial conditions—a knowledge which, it has to be confessed, is still imperfect and incomplete. Though xenopus is here referred to as a toad and by others elsewhere as a frog, actually this anuran belongs to neither of the sub-orders Bufonidae and Ranidae. It is an Aglossan, lacking a tongue and having its two Eustachian tubes united in one median pharyngeal opening in the posterior portion of the palate, and is therefore to be distinguished from both the typical frog and the typical toad (Gadow, 1923). However, it is convenient, even though incorrect, to give to it a popular name. The South African dealer from whom these "toads" were obtained refers to them always as "frogs," but the majority of those who have used them

as experimental animals call them "toads." I propose to refer to them as toads, following the example of Professor Hogben and his colleagues who have written about them.

The Hogben Test

The toads have been used for pregnancy diagnosis, but so far only experimentally, the results obtained being invariably compared with those provided by the Aschheim-Zondek and Friedman tests. I owe much to the willingly given help of Professor Hogben, now of Aberdeen, in whose laboratory in Capetown it was first shown that this anuran might with advantage be used for this particular purpose. I accordingly propose to refer to the biological test for pregnancy in which *Xenopus laevis* is used as the Hogben test, in order to bring it into line with the (mouse) Aschheim-Zondek and the (rabbit) Friedman tests. I am also greatly indebted to Dr. Bellerby and to Mr. Landgrebe, who have been closely associated with Professor Hogben in this work. These investigators have been using xenopus as an experimental animal for different purposes for many years now, and I, being permitted to call upon their experience, have been saved from much disappointment and many a disaster. I imported these toads because I wished to find out for myself whether or not they could replace either the mouse or the rabbit or both in the routine diagnostic work of this laboratory, in which over 10,000 tests are carried out annually. The toad was to be required to show that, as a test animal and under the conditions that exist here, it was as cheap, as convenient, and as trustworthy as the mouse and the rabbit have so abundantly shown themselves to be.

The Friedman and A-Z Tests Compared

Both the rabbit and the mouse have been found to be necessary, for the reason that they possess different but equally valuable qualities as test animals. The Friedman is a "rapid" test, giving a result in twenty-four to forty-eight hours, whereas the A-Z result is not forthcoming until the fifth day. For this reason, in cases in which urgency is the most important consideration the Friedman is the test of choice. The reaction of the rabbit, however, is rather of the nature of an "all or none" response, giving either a definite positive or a definite negative. Though the A-Z is a slow test, the mouse yields with different concentrations of hormones a graded series of reactions ranging from a "strong" positive through the ordinary "standard" to "weak" and "extremely weak" positives, a negative in respect of ovarian effects, but showing very considerable and significant enlargement of the uteri and vaginae to the ordinary unequivocal negative. For this reason the mouse possesses very considerable advantages over the rabbit in respect of its usefulness in those instances in which information other than that relating to the existence or non-existence of normal pregnancy is required. These graded effects, for example, can clearly demonstrate the difference between an exceptionally low hormone concentration and the "normal" concentration in cases of early pregnancy, and thus disclose the threat of imminent abortion. They can also distinguish between true pregnancy and the endocrine repercussions of abnormal emotional states, and between pregnancy and menopausal conditions. They are most useful in that they can record the stages in the recrudescence of chorion epithelioma and hydatidiform mole.

The Toad as a Laboratory Animal

Since it had already been established that in the Hogben test a reliable positive result could be obtained under certain defined conditions within six to fifteen hours after the receipt of the specimen, and since at the time I had no knowledge concerning the possibility of obtaining graded results with the toad, I assumed that if the Hogben test should prove to be satisfactory it might replace the Friedman as a rapid test and leave the A-Z unaffected. The adult rabbit, which has cost about five shillings at the time of its entry into the test, has to be isolated for a month before use, and at the end of the test is killed; the 21-days-old mouse has cost at least sixpence, and is likewise killed. The toad, which has cost eightpence (imported in large numbers) when received into the laboratory, must (except the rare individuals already in breeding condition) be kept for three months and fed thrice weekly on expensive minced beef and liver before it can be primed with an ovulating dose of an anterior pituitary preparation, and then rested for a month before being used for the first time in a pregnancy diagnosis test. It is not killed at the end of the test, but can be used repeatedly after appropriate resting intervals, which should be as long as possible—two to three months after a positive and one to four weeks after a negative test. This procedure of course means that a very large reservoir of animals must be maintained if many tests are carried out daily. A toad that has been used and rested even for two to three months does not need repriming. One great advantage that the toad possesses is that ovulation does not occur in the absence of the male and under laboratory conditions. Ovulation and oviposition are induced only by injection of anterior pituitary and anterior-pituitary-like substances. Spontaneous ovulation in the rabbit, on the other hand, can and does occur in the absence of the male and without any kind of injection. For this reason each rabbit has to be isolated before use and kept in quiet surroundings. The toads require no such care, and can be taken straight from stock into the test.

At one time it was thought (Shapiro and Zwarenstein, 1934) that after a few weeks' residence in a laboratory there was a tendency on the part of the toads to become less useful in these tests owing to ovarian atrophy, but it has since been shown (Alexander and Bellerby, 1935; Elkan, 1938) that this is not the case when a high level of nutrition is maintained. In this laboratory toads that have been in use for eighteen months show no signs of deterioration in respect of their reaction to pregnancy urine. The accommodation required by rabbits and mice is relatively cheap and simple, and the labour involved in their care light. For the toad, however, elaborate aquaria must be provided, the water of which must be changed before and after every meal, and the animal-room itself must be maintained at a constant temperature of not less than 70° F., so that the cost of equipment and maintenance is considerable.

Method of Injecting the Toad

The actual injection of the toad is more difficult than that of the mouse or rabbit. No experience is required to inject the subcutaneous tissue of the mouse's back, and after a few trials the injection of the ear vein of the rabbit becomes a matter of relative simplicity. On the other hand, care as well as experience is required when injecting into the lymph sac of the toad. To have missed the ear vein of a rabbit may mean a spoilt test, but to have missed the lymph sac of the toad usually means the death of the animal. The technique of injection now employed is as follows. The syringe needle (which requires to be rather long and slender) is thrust into the right thigh muscle, not too deeply. It is then directed upwards steadily in a straight course towards the centre of the back to meet the

mid-dorsal line at an angle of 45 degrees. The needle throughout its course should be clearly seen just beneath the skin. When the mid-dorsal line is reached there is an obstructing barrier of connective tissue that has to be passed. When the needle meets this a firm gentle thrust carries it through, beyond, and into the lymph sac. Great care has to be taken to avoid piercing the right lung.

There is, of course, no difficulty concerned in the killing of a 3-weeks-old mouse, but to have to kill a number of adult rabbits is not a pleasant task even though they are anaesthetized by an intravenous injection of nembutal before the neck is broken. My staff would rather deal with the toad that has not to be slaughtered. In spite of the relative expensiveness of its accommodation and management, I am inclined to the view that, all things being considered, the toad can compare not unfavourably in respect of cheapness and convenience with the mouse and the rabbit. However, though these are factors of importance in this comparison, they lose all significance unless it can be shown that the toad is equally trustworthy.

Housing the Toads

The toads are kept, by thirties, in galvanized metal tanks, 4 feet long, 2 feet wide, and 1 foot deep, white-painted on the inside, with water 4 inches deep. A metal rim 6 inches wide overhanging the water on all sides prevents the toads leaping out of the tanks. In one corner of the sloping bottom there is an outlet, fitted with a movable plug; on the under side of the tank this outlet is continued into a pipe on to which a rubber hose can be screwed when the water is to be changed. After the initial priming the toads are fed once a week. They are given as much as they will eagerly accept: one to two heaped handfuls of minced meat or liver is the usual ration for each tank. The meat must be finely minced after all stringy matter has been removed or else the glutton will choke itself. The water is changed on the morning before feeding and again on the day following. A grease trap between the tanks and the main drain is provided to catch the bulky excreta. The tanks are ranged in tiers on staging around the walls of a well-lit room with roof lights, and the temperature is thermostatically maintained at 70° F. by means of electrical tubular heaters. An electric immersion heater in a large cistern within the room ensures that the fresh water entering the tanks during the process of cleaning is of the same temperature as that of the soiled water it is replacing.

Each toad as it enters the test is placed in its own round glass jar, 12 inches high by 8 inches wide, with a neck, with 6 inches of water, in which there is a wire grid platform standing on legs 2 inches high. The eggs, when laid, fall through the platform to the bottom of the jar and so cannot be eaten by the toad. The ova are so adhesive that the greatest care must be taken after each test, not only to wash the wire platform thoroughly, but also to subject it to heat. This latter treatment shrivels up any eggs that remain attached to the grid and thus prevents an incorrect diagnosis being made in a subsequent test. A fine-mesh string net, kept in place by means of a rubber band, covers the top of the jar. The jars are kept in a thermostatically controlled glass-fronted cabinet maintained at a temperature of 75° F. With lower temperatures the duration of the test is increased.

Undoubtedly, factors of the greatest importance in the successful conduct of this particular test are the power of observation and the ability to profit from experience on the part of the human personnel associated with it. It should be mentioned, for example, that the toads that are taken from the large reservoir to enter the test are not just taken at random. They are carefully selected, being to the eye and to the hand such as give the impression of possessing an ovary that will respond; furthermore, they are chosen because they present a cloaca with a bluish or dull-red tinge. A "flat" toad with a pale cloaca is never entered into a test, but is removed from the large reservoir and given extra feeding. If in spite of this it fails to develop the qualities demanded in a test toad it is injected with a known pregnancy urine, and if it does not respond to an ovulating dose is immediately discarded. Toad husbandry is, on the whole, a relatively simple and straightforward procedure, and fuller details concerning the maintenance of reproductive activity for pregnancy diagnosis may be found in the recently published paper by Landgrebe (1939). A condition which is rather perplexing has, however, been encountered. There are now about thirty toads which are suffering from paralysis of the hind legs. This certainly cannot be due to any effect of injection, for there are males among them. It is suspected that this paralysis is due to some deficiency—possibly lack of sunshine. Ultra-violet therapy is now being given. Elkan (1938) has also encountered the same condition during the winter months.

Experimental Results with the Hogben Test

At first the alcohol method used by Zondek and by Shapiro and Zwarenstein was employed in the preparation of the specimen. In this, 300 c.cm. of industrial methylated spirits are added to 100 c.cm. native morning urine (acidulated with glacial acetic acid when necessary) and the mixture allowed to stand for half an hour and then centrifuged. The supernatant fluid is discarded and the precipitate washed with 20 c.cm. ether which is decanted; the precipitate is then dried in a stream of compressed air and thoroughly stirred up with 10 c.cm. of distilled water and centrifuged again on a small hand centrifuge to bring down excess protein. The clear supernatant fluid is decanted and adjusted with sulphosalicylic acid or sodium bicarbonate until a *p*H of 5.5 (approximate) is attained. It is then ready for injection.

The surplus of thirty-one specimens under the Aschheim-Zondek test at the time was used to carry out a parallel series of tests with the toad. Four toads were included in each test, and into the lymph sac of each 1 c.cm. of prepared urine (equal to 10 c.cm. of native urine) was injected. It was assumed that when extrusion of ova occurred within twenty-four hours the result could be accepted as a positive, and that if no such extrusion occurred and the toads were allowed to remain in their jars for a further twenty-four hours, and if at the end of this time no extrusion had taken place, the reaction could be regarded as negative. It is to be noted, however, that these standards differed from those adopted by Shapiro and Zwarenstein, who in cases in which no extrusion of ova had taken place killed the toads and inspected their oviducts. The presence of even one ovum therein was considered to be a positive reaction. Bellerby, on the other hand, demanded for a positive actual extrusion of ova in five out of ten test animals. Neither of these standards could be adopted without considerably increasing the cost of the test.

In thirteen of this series of thirty-one, both the mice and the toads gave a positive result; in thirteen both gave

negative results; whilst in five the results obtained by the two tests were actually or apparently in disagreement (Table I).

TABLE I

Aschheim-Zondek	Hogben (within 48 Hours)	Clinical Confirmation
Extremely weak positive	Negative	Patient miscarried 8 days later
Weak positive	,,	,, pregnant
Strong ,,	,,	,, 6 months pregnant
Weak ,,	1 ovum only	,, 4 ,, ,,
Strong ,,	,, ,, ,,	,, 3 ,, ,,

These divergent results were confusing, particularly in view of the fact that specimens giving a weak and a strong positive A-Z respectively induced the extrusion of but a single ovum in the Hogben test. A result taking the form of the extrusion of one or two ova could never be looked upon as satisfactory, for these can easily be overlooked in the murky water that is present in the glass jar after the toad has been kept therein for several hours. It was therefore decided to inquire whether or not, by modifying the method of preparation of the urine and by using greater concentrations of the hormones, it would be possible greatly to increase the number of ova in cases of this kind. First of all the acetone method of preparation was used instead of the alcohol. These two are similar, except that acetone replaces alcohol and, instead of 300 c.cm. of alcohol, 200 c.cm. of acetone are used with 100 c.cm. of morning urine. A series of 230 tests were made, each of them being checked by the A-Z. In 119 both gave correct positives and in ninety-five both gave correct negatives, but in sixteen—that is to say, in 7 per cent.—there was disagreement, the Hogben test giving a false negative in ten and a false positive in six (Table II).

TABLE II

Aschheim-Zondek	Hogben	Clinical Confirmation
Extremely weak positive	Negative	Case of ectopic gestation
Weak positive	,,	14 days pregnant
,, ,,	,,	4 weeks ,,
,, ,,	,,	4 ,, ,,
,, ,,	,,	3½ mths. ,,
,, ,,	,,	Patient aborted 18 days later
,, ,,	,,	,, pregnant
,, ,,	,,	,, ,,
Positive	,,	,, ,,
,,	,,	,, ,,
Negative	Positive (1 ovum)	Carneous mole
,,	,, (2 ova)	Not pregnant
,,	,, (2 ,,)	,, ,,
,,	,, (2 ,,)	,, ,,
,,	,, (4 ,,)	Tuberculoma of brain
,,	,, (10 ,,)	Menopause

The results obtained showed clearly that this method of preparation and/or degree of concentration were not suitable for adoption as a routine. The percentage of error among the results of the Hogben tests was far too high. The results of the tests when associated with the extrusion of ten or less ova again presented very considerable difficulties in interpretation. It seemed desirable, therefore, to take specimens which had given a "weak" positive A-Z and, by testing out a number of other methods of preparation and using different degrees of concentration, find that method and that concentration which could always be expected to give a clear-cut positive (50 + ova) in the toad (Table III).

TABLE III

Method of Preparation	Concentration of Urine	Amount Injected	No. of Tests Done	No. of Positive Results Obtained
Double-volume alcohol prepn.	10 to 1 c.cm.	1 c.cm.	10	2
,, ,, ,, ,,	20 ,, 1 ,,	1 ,,	20	10
Double-volume acetone prepn.	10 ,, 1 ,,	1 ,,	15	4
,, ,, ,, ,,	20 ,, 1 ,,	1 ,,	51	30
,, ,, ,, ,,	20 ,, 1 ,,	2 ,,	19	15
Acetone prepn., but filtered under pressure instead of centrifuging as recommended by Dr. Bellerby (personal comm.)	20 ,, 1 ,,	1 ,,	6	1
	20 ,, 1 ,,	2 ,,	6	1

The results obtained showed that the acetone method was to be preferred, and that the next and final step in the inquiry should be that of determining what particular concentration gave the best results.

In addition to the above a series of tests was carried out using native urine. Again four toads were included in each test, and 2 c.cm. of urine which had been detoxicated by means of sulphosalicylic acid were injected into the lymph sac of each. Twenty-six specimens which had already given a definite "standard" positive A-Z reaction were used for this series, but in only nine cases was a positive toad reaction obtained, and in most of these fewer than 100 ova were extruded. The fact that native urine proved to be so entirely unsatisfactory is interesting in the light of the experience of others. It may be that the freshness of the specimen and the degree of concentration are factors which are responsible for the difference between our results and those obtained by Bellerby and Elkan, who both found native urine to be satisfactory. If the injection of native urine were to be adopted as the routine procedure in this laboratory it would become necessary to obtain and maintain a stock of toads uniformly remarkable for their continued sensitivity to small amounts of anterior-pituitary-like hormones, and since this is an ideal beyond attainment some alternative procedure had to be elaborated.

At one time it was thought that perhaps the degree of reaction of the toad might be assessed by weighing the whole of the ova expelled, but this was soon found to be quite impracticable, for it was too time-consuming to collect every single ovum from the test-jar and its wire platform, though it is possible to train oneself to form, by inspection, a fairly accurate estimation of the number of eggs present in the jar. These estimations were of course checked by actual counting until, through experience, it was found that they approached the actual numbers closely enough. The maximum number of eggs produced by any toad under test here has been 300+; the great majority of results obtained fall into one or other of the following classes:

Negative: no expulsion of ova from the cloaca up to thirty hours.

Indefinite negative: cloaca dull red with some enlargement; such a toad may ovulate after twenty-four hours.

Extremely weak positive: less than 20 ova (actual number can be counted).

Weak positive: 20 to 50 ova.

Positive: 50 to 200 ova.

Strong positive: more than 200 ova.

The indefinite negative is difficult to classify. The discoloration and enlargement in certain cases are seen actually before injection and are thought to be due to the congestion associated with defaecation. They are seen also during forceful expulsion of ova, particularly in those cases in which the eggs are not extruded until twenty-four hours after injection. The obvious method of distinguishing between the two causes of these phenomena is to kill the toad. This, for reasons that have been stated, is not convenient, and so the practice of this laboratory now, in any case in which ovulation has not occurred at the end of fifteen hours, is to delay decision for another fifteen hours before a definite negative result is returned. It is unnecessary to give a second injection at the end of the first fifteen hours in such cases. In the great majority of instances in which a positive result is obtained the ova make their appearance between five and fifteen hours after injection. If no such extrusion has occurred at the end of fifteen hours the result expected is either a weak positive or else a negative. If the former, then extrusion of ova occurs usually within twenty-four hours after injection.

Two series of tests, all of them checked simultaneously by means of the Friedman and the A-Z tests, were next carried out in order finally to decide which particular concentration should be adopted for routine use. In ninety-three tests in which the concentration was 20 to 1 c.cm., and 1 c.cm. of the extract was injected, there was one disagreement, in which the Friedman gave a positive, the A-Z a weak positive, and the Hogben a negative. This specimen was from a woman aged 40, recently married, whose last menstrual period was twelve weeks past. In eighty tests in which 2 c.cm. of the same concentration (equal to 40 c.cm. of native urine) were injected there was 100 per cent. agreement between the results of all three methods. Two c.cm. would seem to be the greatest amount that should be injected, as, apart from the risk of non-absorption, a larger quantity is not well tolerated by the toads.

A Possible Danger

There is one danger connected with this method of preparation, and that is that the specimen when ready for injection must be absolutely free from the slightest trace of acetone. If not, the toads react by becoming coated with a film of mucus, and most commonly those subjected to this treatment die. It has been found desirable to work with preparations that are not completely neutralized. As in the Aschheim-Zondek so also in the Hogben test: the specimen, when ready for injection, should be very slightly acid, giving an orange colour (*p*H approximately 5.5) when tested with B.D.H. universal indicator. It has been found that if it is completely neutralized, giving a brown reaction, or made even slightly alkaline, it can become quite alkaline before the test is completed, and such alkalinity is undoubtedly the cause of the death of many animals and the ruination of many tests.

In order to determine whether this concentration would give positives when non-pregnancy urine was employed, a series of tests was carried out using specimens which had already given a negative A-Z, and in no case was an incorrect positive obtained. It was accordingly decided that 2 c.cm. of a 20 to 1 c.cm. concentration, using the acetone method of preparation, should be adopted as a routine procedure. The adoption of the Hogben test demands that, in place of the 30 c.cm. of early morning urine required for the combined Friedman and Aschheim-Zondek tests, not less than 200 c.cm. shall be made available.

Conclusion

It appears that, under the conditions which exist here, the toad can be as trustworthy as the mouse or rabbit when the specimen is properly prepared. Such preparation is undoubtedly much more expensive in time and money than is that required for the Friedman and the A-Z, but the actual expense can be considerably reduced by redistillation of the acetone.

The toad can certainly replace the rabbit in these tests. It can give a trustworthy positive result within six to fifteen hours, as compared with the twenty-four to forty-eight hours of the rabbit. The toad is better than the rabbit for use in those cases in which a definite positive or an emphatic negative is the answer to the question asked, and especially in which the result is urgently required. With the present methods of preparation, however, the toad does not give the graded results yielded by the mouse. It may be shown later that differences in the number of eggs extruded are indeed true reflections of corresponding differences in hormone concentration, but for the present the mouse must be retained.

Summary

1. It is proposed to name the biological pregnancy diagnosis test in which *Xenopus laevis* is used the Hogben test.

2. The Hogben test can at once replace the Friedman but not the Aschheim-Zondek test.

During the course of this study I have depended greatly upon the unfailing loyalty and technical skill of the staff of this laboratory, and to them, and especially to Miss Mairi Mackay and Miss Janet Meikle, I wish to acknowledge my great indebtedness and to express my thanks.

BIBLIOGRAPHY

Alexander, S. S., and Bellerby, C. W. (1935). *J. exp. Biol.*, **12**, 306.
Bellerby, C. W. (1934). *Nature*, **133**, 494.
Elkan, E. R. (1938). *British Medical Journal*, **2**, 1253.
Gadow, H. (1923). *Amphibia and Reptiles*, Cambridge Natural History, London.
Hogben, L. (1930). *Proc. roy. Soc. S. Afr.*, March.
Landgrebe, F. W. (1939). *J. exp. Biol.*, **16**, 89.
Shapiro, H. A., and Zwarenstein, H. (1934). *Nature*, **133**, 762.
—— —— (1935). *S. Afr. med. J.*, **9**, 202.
Zondek, B. (1930). *Klin. Wschr.*, **9**, 964.

At the annual meeting of the Governors of St. Mark's Hospital, City Road, E.C., the Lord Mayor of London (Sir Frank Bowater), in moving the adoption of the annual report, said that the space set free by the erection of the nurses' home, opened in May last by the Princess Royal, is to be utilized to improve the operating theatre unit and increase the accommodation for public and private ward patients. Work on the second operating theatre will be completed early during the current year. The report stated that in view of the fact that the value of *x* rays in assisting diagnosis is becoming more and more evident, steps are being taken to enlarge the accommodation and install modern equipment. As a result of the follow-up scheme for cancer cases a systematic record is now available of all cases admitted to the hospital from 1910 to date. On the basis of detailed pathological examination, tumour cases have been grouped into three classes: Group "A," or early operable; Group "B," in which the muscle wall of the bowel has been penetrated by the growth; and Group "C," in which extension outside the rectum has occurred, with invasion of the regional lymph nodes by the growth. Results indicate that approximately one-fifth of the cases fall into "A"; Group "B" constitutes about one-third, and "C" (late operable) forms practically one-half, of the cases. In the past few years the operability rate has slowly risen to a figure of 68 per cent.

THE SIGN OF THE CLAVICLE

BY

E. CUNNINGHAM DAX, M.B., B.Sc.
Assistant Medical Officer, Leavesden Hospital

AND

R. M. STEWART, M.D., F.R.C.P.Ed.
Medical Superintendent, Leavesden Hospital

In two recent papers Higouménakis (1930, 1937) has called attention to an enlargement of the sternal end of one clavicle in congenital syphilis, and he considers that its presence is a valuable aid in the recognition of this disease. He explains the development of this enlargement as a hyperostosis resulting from an osteitis in which three factors are concerned.

1. *Anatomical.*—The clavicle has a different mode of development from the other long bones, the shaft being ossified by the fusion of two centres which appear in connective tissue without an intermediate cartilaginous stage. This fused centre in the shaft is laid down early in foetal life, but the secondary centre at the sternal end does not appear until the eighteenth or twentieth year, joining the shaft at the twenty-fifth.

2. *Biological.*—The *Spirochaeta pallida* has a predilection for connective tissue, and it would thus be expected to settle in this bone, in much the same way as it chooses the membranous bones.

3. *Mechanical.*—The continual strain to which the inner end of the clavicle is subjected by movements of the arm tends to give a unilateral distribution to the lesion, corresponding to the right- or left-handedness of the individual.

Higouménakis defines the lesion as an increase in volume of the sternal end of the clavicle, particularly of the inner third. He found the sign to be present in more than 85 per cent. of the 197 cases which he had collected up to 1930. In his more recent paper he gives an account of another twenty patients and mentions a further seven shown at the Society of Medicine of Athens in 1930 and 1931; in all of these the lesion was present. He quotes five observers who have shown nine other cases in Athens and in Cairo. Dorne and Zakon (1935) also found the sign in each of twelve undoubted cases of congenital syphilis. Touraine *et al.* (1932, 1933) described a case with an enlarged clavicle as a localized form of von Recklinghausen's disease of bone, but later, as the result of a biopsy, they gave evidence that long-standing syphilitic changes were present in the enlarged portion. In consequence of his own clinical experience and of these confirmatory observations Higouménakis states that the sign has a diagnostic value equal to the cardinal signs of congenital syphilis. Its frequency, he says, is greater than the other certain stigmata of the disease, hence its diagnostic value; furthermore, it is never found in the subjects of acquired syphilis or in the normal.

Stokes (1934), on the other hand, quotes the investigation of a series of 202 cases of late congenital syphilis at the Mayo Clinic in which the clavicle was found to be enlarged in only 4 per cent. of the patients. It would appear that he considers the sign to be a useful adjunct in the diagnosis of congenital syphilis, but he classes it as "a secondary landmark," by itself insufficient for diagnosis. He remarks that, while clavicular enlargement is not common, it is often overlooked, and that, when present, gross thickening of the inner end of the clavicle with roughening and irregularity should be checked by *x*-ray examination. Moreover, he lays stress upon the examination of the shoulder girdle in acquired syphilis, adding that enlargement of the sterno-clavicular articulation has been recognized in this condition for years past. Thus, of 239 cases with late bone and joint lesions in acquired syphilis seen at the Mayo Clinic, 7.5 per cent. showed clavicular enlargement. In their twelve cases of congenital syphilis, Dorne and Zakon (1935) refer to the enlargement as being of the sterno-clavicular articulation, though Higouménakis denies this on radiological grounds. Stafford McLean (1931), writing on the *x*-ray appearances of the bones in congenital syphilis in children, states that, although the examination of the clavicle was not a routine, an enlargement of the body was found in a number of cases.

The Present Investigation

Nearly 2,000 of the patients in this hospital are mental defectives. Among them there were sixty-four cases of congenital syphilis, and these have been re-examined to ascertain the incidence of clavicular abnormality. As controls, the clavicles of 1,200 non-syphilitic patients have been examined. It must be observed that palpation gives more information as to size and shape than a single *x*-ray film, and misleading results may be obtained by reliance on inspection and photography, which for their success depend partly on the absence of subcutaneous fat. Furthermore, since there appear to be variations in the form of the clavicular enlargement the sign has not been found simple to elicit. From an analysis of our material we have been able to differentiate five types of abnormality, of which two or more may be found in the same patient: (1) a generalized enlargement of the sternal end; (2) an exostosis projecting from the inner end of the bone; (3) a roughening of the inner third; (4) a thickening of the posterior surface of the outer third or an enlargement of the middle of the bone; (5) a tubercle at the point of origin of the clavicular head of the sterno-mastoid muscle. If any of the first three abnormalities were present to a marked degree in a case of congenital syphilis we have classed this as a positive result. Minor deformities we have found to be so common, even among normal people, that no account is taken of them in our results.

Of the sixty-four patients, twenty-nine are females and thirty-five males, their ages ranging from 18 to 73 years. Fifty-six have had positive serological reactions within the last seven years; their chances of having acquired syphilis can be eliminated either by their length of stay in institutions or by the presence of unequivocal stigmata. The following table shows the clinical evidence on which the diagnosis of congenital syphilis was made in the eight cases with negative serological findings.

Age and Sex	Facies	Interstitial Keratitis, Choroido-retinitis, or Iritis	Hutchinson's or Moon's Teeth	Osteo-periostitis	8th Nerve Deafness	Rhagades	Family History
35, F.	+	+	Edentulous	+	+	+	−
42, F.	+	+	,,	+	+	−	+
38, F.	+	−	,,	+	−	−	+
50, F.	+	+	−	−	−	+	+
35, F.	−	+	+	+	+	+	Not known
26, M.	−	+	−	−	+	−	+
36, M.	−	+	+	+	−	−	−
37, M.	+	+	+	+	−	+	−

Of the sixty-four cases of congenital syphilis, nineteen (ten males and nine females) show gross abnormality of

the inner ends of their clavicles, in the form of general enlargement, exostoses, or roughening. The 1,200 controls represented examples of all degrees of mental defect. Thirty-nine of the 600 males and twenty-six of the 600 females examined have marked clavicular enlargement similar to that observed in the cases of congenital syphilis. No obvious relation between the hand used and the enlarged clavicle was found to exist in either the patients with congenital syphilis or the control group—in fact, in the small majority of cases the larger clavicle was on the left side, though most used their right hands by preference. We were unable to find a single case of congenital syphilis with a gross enlargement on the one side that was not accompanied by some degree of deformity on the other.

Results

Possible explanations for the difference in our results from those recorded by Higouménakis and others are:

1. The sign may be produced in its fully developed form only when the patient has performed a considerable amount of manual labour. If this is so, one would not expect to find clavicular enlargement in other than a very small percentage of our cases, the majority of whom are incapable of sustained work.

2. All the controls with enlargements might be unrecognized cases of congenital syphilis without other stigmata or positive serological reactions. This does not seem to be remotely possible; even if it were, the bilateral distribution of the lesions would still demand explanation.

3. The enlargements we have found may be of a different type from those described by Higouménakis, in which case the sign would obviously have little value as a stigma of congenital syphilis. On the other hand, if the lesion we have seen in nineteen of our cases of congenital syphilis is identical with that described by Higouménakis, its distribution is certainly not unilateral, and since it can occur in normal controls it cannot be accepted as reliable evidence of congenital syphilis.

Lesné and Boutelier (1931), in discussing the relation of syphilis to the bony changes of rickets, quote Cannuta's figures: of 1,285 cases of rickets 37 per cent. showed definite evidence of congenital syphilis. From this it would appear that the two diseases are often associated, and we think it not improbable that the tubercles observed at the points of origin of the clavicular heads of the sterno-mastoids may be rachitic in origin. By far the largest tubercles we have seen were in a rickety dwarf. The tubercles were found about as often as the clavicular enlargement both in the cases of congenital syphilis and in the controls. These two abnormalities were associated frequently enough to suggest a common factor in their production, which seemed more likely to be rachitic than syphilitic.

The remaining two types of deformity noted in our series were found to be so uncommon as hardly to merit description, but a brief reference to two examples may be of some interest. A juvenile general paretic aged 26 shows a regular spindle-shaped enlargement of the middle of each clavicle with normal sternal and acromial extremities. A congenital syphilitic female aged 35, with cranial bossing, tibial thickening, and large exostoses of the sternal ends, exhibits a marked thickening of the posterior borders of the outer thirds of both clavicles; this thickening was also observed to a lesser degree in two other cases of congenital syphilis.

Summary

Higouménakis has claimed that "the sign of the clavicle" is one of the most certain and common stigmata of congenital syphilis. We have examined sixty-four patients with the disease and found enlargement of the sternal ends of the clavicles in nineteen (29.7 per cent.). Among 1,200 non-syphilitic controls similar lesions were found in sixty-five (5.4 per cent.).

We have not been able to show a relation between the degree of clavicular enlargement and the right- or left-handedness of the patient. In our cases the lesions were never strictly unilateral.

For the *x*-ray examination of the patients we are indebted to Dr. H. Glynn Hall.

REFERENCES

Dorne, M., and Zakon, S. J. (1935). *Arch. Derm. Syph.*, Chicago, **32**, 602.
Higouménakis, G. C. (1930). *Dtsch. Z. Nervenheilk.*, **114**, 288.
—— (1937). *Ann. Derm. Syph.*, Paris, **8**, 939.
Lesné, E., and Boutelier, A. (1931). *La Syphilis Héréditaire Larvée*, Paris.
McLean, S. (1931). *Congenital Osseous Syphilis*, Chicago. (Reprinted from *Amer. J. Dis. Child.*, 1931.)
Stokes, J. H. (1934). *Modern Clinical Syphilology*, 2nd ed., Philadelphia.
Touraine, Ribadeau-Dumas, C., and Lortat-Jacob, E. (1932). *Bull. Soc. franç. Derm. Syph.*, 1641.
—— —— and Bernou (1933). Ibid., 120.

BRONCHOGRAPHY WITHOUT COCAINE

BY

ERIC C. O. JEWESBURY, B.M., M.R.C.P.

Chief Assistant to a Medical Unit, St. Bartholomew's Hospital; and Exchange Fellow in Medicine, Pennsylvania Hospital, U.S.A.

The introduction of lipiodol, neo-hydriol, or other radio-opaque oil into the bronchial tree as an aid to radiological investigation of certain diseases of the chest is becoming a more and more frequent procedure. Varying methods employed are capable of giving satisfactory results in practised hands. None the less, the degree of discomfort the patient may experience varies with each method, and it is for this reason that certain modifications of technique are to be preferred.

Usual Methods

When a gum-elastic catheter is inserted by the nasal route it is necessary first to spray the nose and throat with cocaine solution. This procedure, though simple enough, often causes considerable discomfort. It may even cause collapse if the patient is sensitive to cocaine. After an interval to allow the nose and throat to become anaesthetized the catheter is passed through the nose and down the larynx. The ease with which this is performed will depend upon the patient in question and upon the skill of the manipulator. There may be difficulty in passing the catheter through the glottis; the patient may choke violently and try to pull out the tube, which may even pass into the oesophagus. Though the method is satisfactory in skilled hands, it is not always easy and not always without distress to the patient.

Instead of passing a catheter by the nasal route it has been considered simpler to spray the back of the throat with cocaine and inject the oil over the base of the tongue. This is certainly more comfortable for the patient, but the preliminary cocainization is often complained of as being more unpleasant than the injection of the oil itself; the patient usually has to hold out his own tongue throughout, and although the lower lobes can be filled well it is not easy to fill middle or upper lobe bronchi by this method. Not infrequently some oil passes down the oesophagus instead of the larynx.

The third chief method is by injection of oil direct into the trachea through the crico-thyroid membrane, and this is the most reliable method because there is neither the difficulty of the first method in getting past the glottis nor the risk of the second method of injecting it into the oesophagus. It is possible to fill all main bronchial divisions by alterations of posture. For the insertion of the trocar and cannula (preferably the Chandler pattern) the patient lies on his back and a few minims of novocain are injected subcutaneously in the midline between the upper margin of the cricoid and the lower margin of the thyroid cartilage. The trocar and cannula are then pressed firmly through the membrane, the trocar withdrawn, the patient sat upright, and the oil injected, making sure by preliminary withdrawal of bubbles of air that the cannula is within the trachea. This method is simple, reliable, and without discomfort to the patient.

Previously it has been considered necessary to inject 5 or 10 per cent. cocaine solution through the crico-thyroid membrane before inserting the trocar and cannula, and, as in the previous methods, this may produce unpleasant symptoms, with faintness and very rapid pulse. Alternatively, preliminary tests for sensitivity to cocaine are necessary, a record of the patient's reactions and pulse rate for half an hour after the subcutaneous injection of 0.1 c.cm. being required. After cocaine has been used in the throat the patient must be instructed not to attempt to swallow food or drink for two hours, for fear of this passing into the insensitive larynx. Moreover, the sudden injection of the cocaine solution itself frequently initiates a temporary fit of coughing. Maxwell (1938) remarks that "the needle must be withdrawn immediately, as there is always a reflex cough and it is possible that the needle might be broken."

Anaesthetization of the Trachea

Is anaesthetization of the trachea necessary? Ever since Sir James Mackenzie's (1909) statement that the respiratory tract below the larynx seemed to be incapable of originating the stimulus for cough production varying views have been expressed. Larsell and Burgett (1924) have shown that in the course of bronchoscopy mechanical stimulation of the lower respiratory passages, including the carina and bronchi as far as those of the second order at least, elicits a marked cough, but Chevalier Jackson (1922) has noticed that in a large number of broncho-pulmonary diseases the cough reflex from contact with the bronchoscope seems rather diminished than increased. It is notorious that the inhalation of a foreign body is likely to set up a violent paroxysm of coughing as the foreign body enters the larynx and passes through the glottis, but once it is beyond this "laryngeal sentinel" it soon ceases to cause any coughing unless it is very movable.

Such considerations throw doubt on the necessity or desirability of injecting cocaine into the trachea when introducing oil by the crico-thyroid route. In the last fifteen instances where I have introduced oil by that route I have therefore deliberately used no cocaine. The only local anaesthetic employed was the small amount of subcutaneous novocain, and this did not penetrate the crico-thyroid membrane. In none of these cases did the patient complain of pain or discomfort and in none of them did the introduction of the oil (neo-hydriol) set up either irritation or coughing. One patient who had an irritative cough at the start took oil without effect on her coughing and was subsequently given cocaine, which quieted her.

Conclusions

It would thus appear that in many respects the crico-thyroid route without cocaine is the simplest, the most certain, and the speediest. It is undoubtedly much the most comfortable for the patient. Only a small needle-puncture wound is produced; this is covered for a day or so with a wisp of wool and a narrow piece of elastoplast not more than one inch long.

The advantages of using no cocaine are: (1) there is no necessity for previously testing the susceptibility of the patient to cocaine; (2) there is no risk of faintness and rapidity of pulse such as not infrequently occur in those who show no marked reaction to the subcutaneous test dose; (3) the patient can eat and drink immediately afterwards; (4) there is no coughing or risk of a broken needle; (5) it is a quick and simple method for out-patients.

REFERENCES

Jackson, Chevalier (1922). *J. Amer. med. Ass.*, **79**, 1399.
Larsell, O., and Burgett, G. E. (1924). *Amer. J. Physiol.*, **70**, 311.
Mackenzie, J. (1909). *Symptoms and their Interpretation*, London.
Maxwell, J. (1938). *Introduction to Diseases of the Chest*, London.

INSULIN AS A DRESSING FOR CHRONIC INDOLENT ULCERS

BY

A. R. HUNTER, M.B., Ch.B.

Resident Medical Officer, Falkirk and District Royal Infirmary

The use of insulin as an external dressing is by no means to be regarded as an innovation; Lawrence has mentioned its employment in 1926 by certain French workers. Fresh interest in the subject has, however, recently been aroused by the writings of Leyton (1938), who states that epithelialization of chronic skin wounds proceeds very rapidly when insulin is used as a dressing. With a view to the confirmation or otherwise of this statement it was considered desirable in the present investigation to secure a more accurately controlled series of cases.

The Present Investigation

With the help of Messrs. Burroughs Wellcome's technical and scientific department two solutions were prepared, the first being a solution of insulin of strength 40 units per c.cm., and the second a control solution containing the same percentage of tricresol, of the same *p*H and osmotic value. Wherever possible two similar wounds in the same patient were dressed in the same way. If that was not feasible the control solution was applied first. Thereafter an application of the insulin proper was made. In every case the solutions were used in the form of a "wet" dressing; for it is unlikely that the concentration could be maintained in the ulcer area if the insulin were allowed to soak into the covering layers of gauze and bandage. Details of the cases studied are as follows.

Case 1.—A typical callous varicose ulcer in a neglectful patient. Fourteen days' dressing with the control solution produced satisfactory granulation and healing. Soon, however, this dressing ceased to be an effective stimulant of epithelial growth. Fourteen days' further treatment with insulin did not alter the now slowed rate of healing, but at a later date a change to Hey's wash (lotio rubra) produced satisfactory and complete closure.

Case 2.—A varicose ulcer, about three inches in diameter, over the anterior aspect of the tibia, with a yellowish sloughy

base and no evidence of vital reaction, in a woman aged 78. Application of the control solution resulted in immediate discharge of much pus, clearance of slough, and some signs of epithelial regeneration. Insulin dressing produced a similar effect but no increase in the rate of epithelization.

Case 3.—A plaster sore in a patient aged 13 transferred from a distant hospital in a hip plaster. This was dressed for one week with control solution and for one week with insulin. No difference in the rate of epithelization was noted. The slough separated speedily with the control solution.

Case 4.—A callous ulcer in a burn scar in the axilla of a child aged 4. Control solution was applied, with satisfactory granulation and epithelization. Four weeks later epithelization slowed down to a marked degree, although granulations required the application of bluestone. At this stage insulin failed to produce epithelial reaction.

Case 5.—Avulsion of most of the tissues round the knee-joint with scarring and callous ulcer formation—the late result of a road accident to a boy aged 11. There were two pairs of similar areas on this leg. One of each of these pairs of areas was dressed with insulin and the other with the control solution. Over a period of three weeks healing was unsatisfactory with both dressings. A change was then made to Hey's wash, and epithelization started satisfactorily.

Case 6.—Cellulitis of dorsum of foot, leaving a wide raw area, in a patient aged 74. The region was dressed for ten days with the control solution and for ten days with insulin. Healing was satisfactory with both dressings. As at one period it appeared that the whole foot would become gangrenous it is surprising that healing occurred at all. Exuberant granulation was not observed in this case.

Case 7.—Burn of dorsum of foot caused by molten metal. Here there were two areas of epithelial loss. One was dressed with control solution and one with insulin. In both healing was satisfactory and proceeded at the same rate.

Case 8.—This refers to the same patient as Case 7, but at a date two months later, when scarring and neglect had led to loss of epithelium and the development of overhanging edges and a sloughy lifeless base. Both insulin and control solutions were tried and both failed. No other dressing used hitherto has proved satisfactory.

Case 9.—Chronic ulcer of the thoracic wall following the application of deep *x* rays to an area from which a breast cancer had been radically removed. At this period insulin only was available, and a control was provided by a similar case treated with saline dressings. At the beginning of treatment the skin edges were rolled and the base of the ulcer was purely slough, but after about three weeks some evidence of a healing epithelial edge was seen. The patient defaulted long before healing was complete. The control case showed healing at much the same rate, but was not entirely satisfactory as a control since this patient was the more robust of the two.

Case 10.—Callous ulcer of the buttock following a carbuncle which discharged its slough and failed to heal. The patient was a diabetic. Control solution produced in forty-eight hours the signs of extending ulceration, the skin edges becoming ragged and the surrounding tissues inflamed. After a few days' boric acid dressing, which restored the ulcer to its original callous condition, insulin was tried, but its effect was similar to that of the control solution.

In chronic cutaneous sepsis the epithelial cells are deprived by the subjacent fibrosis and endarteritis of their normal supplies from the blood stream. In the absence of these supplies it seems reasonable to assume that the epidermal cells will develop a preponderantly glycolytic metabolism such as Warburg (1930) has described in tumours, and with this metabolism lose the power of regular growth. The theoretical basis for the use of insulin in such cases is that this substance might be expected to stimulate aerobic metabolism and thereby restore growth to its previous regularity.

Conclusions

In practice, however, the following conclusions were reached:

There is no serious evidence of any injurious local effect from the use of insulin as a dressing agent. (Case 10 is exceptional in that the patient died two days after the test from chronic pyaemia which had lasted almost three months.)

In nearly all cases some progress of the wound towards healing occurred.

With two of the patients (Cases 1 and 4), when the control solution had ceased to stimulate epithelial growth the use of insulin produced no further reaction such as would be expected from a change of dressing.

Where an ulcer had a sloughy base one of the first reactions was a profuse purulent discharge with both dressings.

There was never any evidence of systemic absorption of insulin—that is, no hypoglycaemic symptoms were observed—and accordingly it was not considered necessary to investigate blood-sugar levels.

In general it may be concluded that insulin is no more satisfactory as an external dressing than a similar solution of tricresol containing no insulin.

I have to acknowledge with thanks the supply by Messrs. Burroughs Wellcome of a solution of insulin and a control solution for use in this clinical experiment.

BIBLIOGRAPHY

da Costa, S. F. Gomes. *Tumori*, **6**, 140.
—— and Guedes, F. Bénard (1933). *Acción med.*, **4**, 442.
Handley, W. Sampson (1931). *The Genesis of Cancer*, London.
Lawrence, R. D. (1938). *British Medical Journal*, **1**, 143.
Leyton, Nevil (1938). Ibid., 70.
Warburg, O. (1930). *Metabolism of Tumours*, London.

Clinical Memoranda

Mumps as a Possible Complication of Chronic Pancreatitis

Acute pancreatitis is a recognized complication of "mumps," but I have been unable to trace a recorded case of "mumps" occurring *after* pancreatitis. The following case history is therefore of some interest.

CASE RECORD

A woman aged 77 was admitted to hospital on October 14, 1938, after three weeks' illness. She had had a "cold," followed in one week's time by sharp intermittent pain below the right scapula and in the right hypochondrium. The skin had become progressively more jaundiced, the stools were clay-coloured, and there was nausea but no vomiting. No loss of weight was admitted, and she had never had jaundice previously.

On admission moderate jaundice of skin and conjunctivae was present, but there was no elevation of temperature and the pulse rate was 90. The gall-bladder was distended and easily palpable almost down to the level of the umbilicus, and the liver was palpable an inch below the costal margin. The stools were clay-coloured and the urine contained bile. Clinically, carcinoma of the head of the pancreas was the diagnosis. While in hospital the jaundice became progressively deeper and the gall-bladder bigger. Radiographs showed no stones. The temperature became slightly raised in the

evenings, and the pulse rate was 90 to 100. In view of her age and general condition no operative treatment was attempted. On October 28—that is, fourteen days after admission—the temperature rose to 100° and the right parotid gland became enlarged and tender. Two days later the left parotid gland was similarly affected, and she presented the clinical picture of "mumps." Associated with the "mumps" was a stomatitis and injection of Stenson's duct orifices. The temperature then fell to subnormal, and she died from cholaemia with intense olive-green jaundice on November 3.

Post-mortem Examination.—The gall-bladder and common bile duct were greatly distended down to the ampulla of Vater, where there was a ring constriction, but no gall-stones were found in the gall-bladder or bile ducts. There was no carcinoma of the head of the pancreas, but histologically the ring constriction was due to a small adenocarcinoma, and the whole of the pancreas showed chronic interstitial pancreatitis.

COMMENTS

Carcinoma at the lower end of the common bile duct is somewhat rare. In this case there was a rapidly fatal issue within four weeks of the onset of the jaundice, and typical infective parotitis was a terminal feature. "Mumps" at the age of 77 is uncommon. It is possible that the patient may have been incubating the disease before admission, but there were no other cases in the household.

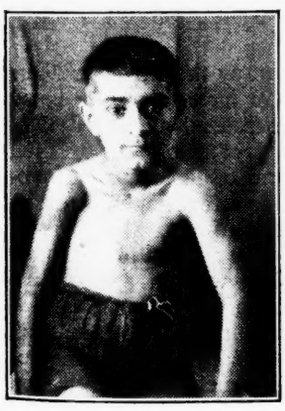
Histologically the carcinoma was small and the whole pancreas showed chronic interstitial pancreatitis. The most probable explanation seems to be that the two conditions of malignancy and infective parotitis followed chronic inflammatory disease in the pancreas.

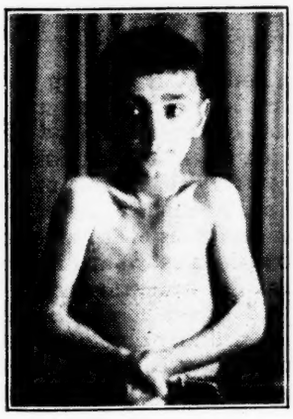
A. WILSON RUSSELL, M.D.,
Deputy Medical Superintendent, St. Chad's Hospital, Birmingham.

Congenital Absence of Pectoral Muscles on One Side

I hope this case will be of interest, especially to anatomically minded members of the profession.

The boy is an Assyrian aged 10, normal and athletic (footballer and runner). Apart from this anomaly he is physically fit and mentally alert, being a good mathematician. He is right-handed. His parents are alive and are





normal and healthy. Neither of them shows this defect, nor does his younger and only brother, 3 years old. I have also examined all his living relatives, with negative results. So far as they know, none of their ancestors had this anomaly. The idea of all this scrutiny was to eliminate the possibility of the defect being hereditary and familial.

Mosul, Iraq. MALCOLM B. PARHAD, M.B., Ch.B.

Infectious Pneumonia

It seems probable that chemotherapy will be extensively, and perhaps exclusively, used in the treatment of cases of pneumococcal pneumonia. The following series of cases illustrates in a somewhat dramatic fashion the results that may be obtained with early serum treatment.

CASE RECORDS

Case 1.—A farmer aged 45 developed lobar pneumonia and died on the seventh day of disease. Throughout the course of his illness he coughed up abundant "rusty" sputum.

Case 2.—A nurse aged 20, who came into contact with him for only twelve hours, developed lobar pneumonia forty-eight hours later. Her temperature fell by crisis on the seventh day and she made a good recovery.

Case 3.—A brother-in-law aged 35, whose contact with the first patient was limited to two or three occasions, developed a lobar pneumonia forty-eight hours after the funeral. He had assisted in laying out the corpse. He had not been in contact with the first patient for some days previous to his death. He was moved to a nursing home eighteen hours after the onset of his illness, and was by then already coughing up abundant sputum.

Case 4.—The next day—that is, thirty-six hours after the onset in Case 3—the widow (aged 46) of the first patient had a rigor and her temperature rose to 104°. She had assisted in nursing both the first and the third cases.

TREATMENT

In Case 1 no specific treatment was given and no bacteriological examinations were made. Case 2 was nursed in hospital, not under my care. No specific treatment was given. Examination of the sputum showed pneumococci, but these were not typed. In Case 3 the sputum showed pneumococci, Type II. This patient was given 60,000 units of Felton's serum intravenously on the second day of disease. During the following twelve hours his temperature fell by crisis, and he made an uninterrupted recovery but for a rise in temperature four days after the crisis, when a fresh patch of consolidation with a pleural rub developed.

When first seen Case 4 showed no local signs. Respirations were not increased and there was no pain in the chest. Viewing the sequence of cases, I regarded hers as a case of pneumococcal infection. She was moved to a nursing home and given 60,000 units of Felton's serum intravenously. Her temperature fell by crisis during the next twelve hours. The following day an impaired note and moist signs were observed in the left base, and there was slight pain, but no signs of consolidation. About this time she produced a small amount of sputum. On the third day after the crisis her temperature rose to 101° for twelve hours. The next day she had a pleural rub and definite signs of consolidation at the affected base. She made an excellent recovery. Her sputum was not examined.

DISCUSSION

It is regretted that full bacteriological examinations of sputa were not made in this series of cases. Clinically there was no doubt that all four patients suffered from the same infection. The unusually high infectivity is difficult to explain. There was no evidence to suggest a coincident influenzal infection. The only significant fact is that both the first and third cases, from whom presumably the second and fourth cases respectively derived their infections, produced sputa in an amount that was unusually abundant. When the almost total lack of precaution that is common in nursing cases of pneumonia is remembered, it is perhaps fortunate that the sputum of the typical case is so scanty in amount.

The value of serum treatment rests on secure if somewhat statistical foundations. To those doubters who find statistical analysis difficult, and who are not unduly critical, this small series may prove convincing.

Wolston, Coventry. G. CAMPBELL, B.M., B.Ch.

Reviews

AIR RAID CASUALTIES

Medical Organization and Surgical Practice in Air Raids. By P. H. Mitchiner, C.B.E., F.R.C.S., and E. M. Cowell, D.S.O., F.R.C.S. Foreword by Sir Cuthbert Wallace, Bt., K.C.M.G., C.B., F.R.C.S. (Pp. 247. 10s. 6d. net.) London: J. and A. Churchill Ltd. 1939.

Mr. P. H. Mitchiner and Colonel E. M. Cowell have produced a book which, unfortunately, would seem to be timely. To be forewarned is to be forearmed, and the authors deserve congratulations on their successful attempt to inform the medical profession of what is to be expected of it in the event of air raids. The two main problems are administration and treatment, and in dealing with the former the authors state that they have tried to give "the correct organization as at this time envisaged by the Ministry of Health." They point out, however, that this organization has been in process of change, and that further modifications of it may be in force by the time their volume is published.

The chapters on treatment are especially valuable in that the authors have taken full advantage of the experience gained in the Spanish war. Colonel Cowell visited Spain last year so as to gain first-hand information of modern air raid casualties. And as both he and Mr. Mitchiner had extensive experience in the last war, what they have written will be a great help to those for whom the book is primarily written—"a new generation who have no knowledge of wound treatment, management of large numbers of casualties, or actual air raids." There is a brief account of such raids. The modern heavy bomber can fly at a height of 20,000 feet at a speed of 250 m.p.h. or more and carry two to four tons of bombs. One of the effects of the high-explosive bomb is blast. The effect of blast on brick buildings is momentarily to push the walls in and then to suck them out. It is not therefore surprising that blast will kill people without their being actually wounded. Post mortem the lungs are found to be torn and the hollow abdominal viscera ruptured.

Well-chosen illustrations accompany the chapters on the principles of wound surgery, shock, transfusion, infection, and injuries to the various regions of the body. Appendices on first aid and transport, surgery and chemical warfare, equipment for mobile surgical units, and mobile *x*-ray units complete a most useful volume.

TISSUE AND ORGAN CULTURE

Methods of Tissue Culture. By Raymond C. Parker, Ph.D. With a Foreword by Alexis Carrel, M.D. (Pp. 292; 109 illustrations. 24s. net.) London: Hamish Hamilton (Medical Books). 1938.
The Culture of Organs. By Alexis Carrel and Charles A. Lindbergh. (Pp. 221; 111 illustrations. 18s. net.) London: Hamish Hamilton (Medical Books). 1938.

The publication from Carrel's laboratory of two volumes devoted mainly to the all-important question of technique is an event which must interest all those who practise tissue culture. Raymond C. Parker's *Methods of Tissue Culture* is a detailed and well-illustrated exposition of the technique employed by Carrel and his associates, beginning with the elements of equipment, sterilization methods, and the preparation of the nutritive fluids used, and proceeding through the actual handling of explanted tissues to questions of interpretation and the application of the method to morphological and pathological problems. There is a concluding list of selected and classified references, which occupies over forty pages. *The Culture of Organs*, by Alexis Carrel and Charles A. Lindbergh, deals similarly with an altogether new technique, regarded by the authors as having immense possibilities. By means of Lindbergh's pulsating perfusion pump it has been found possible to maintain entire organs alive *in vitro* for periods of up to forty days. Although these have usually been removed from animals, fragments of human organs and tumours have also been treated successfully. Although it seems that most of the viscera and even parts of limbs can be kept alive *in vitro*, a favourite subject of study has been the thyroid gland, both because of its convenient size and vascular supply, and because it responds demonstrably by changes in structure or secretion to appropriate variations in composition of the perfusing fluid. The latter is usually a mixture of serum and Tyrode's solution, but requirements vary, and the kidney, for example, requires blood. These organs not only remain alive in the sense that their cells will continue to grow if explanted, but they function; the kidney concentrates urea, the pancreas secretes insulin, and the thyroid responds to thyrotropic hormone by an increased secretion of iodine. As a technical achievement these studies are remarkable, but the authors' expectation of their future usefulness may seem a little sanguine. Clearly experimentation with individual organs has certain advantages over using the whole animal, and it is just conceivable that light may be thrown on metabolic problems such as the source of the serum proteins and the part, if any, which they play in the nutrition of different organs. Something may also emerge from a study of the healing of wounds or the process of inflammation when the composition of the "blood" supply can be varied at will. With other prophecies made here it is more difficult to agree; the time is surely far distant when a diseased human organ will be excised, treated *in vitro* until cured, and finally replaced! This is an item under the heading of "The Ultimate Goal," and one feels that the authors are to be congratulated on their powers of imagination as well as on their immense enterprise and technical skill.

FEVER THERAPY

Pyrétothérapie. By Charles Richet, Jean Surmont, and Pierre Le Gô. (Pp. 192; 28 figures. 50 fr.) Paris: Masson et Cie. 1938.

In writing this book on fever therapy the French authors Drs. Richet, Surmont, and Le Gô have taken the broad view of the subject and have correlated a great deal of information dealing with the production of artificial pyrexia. The success of this form of therapy when produced by electrical means has led to a focusing of attention on the claims of the different forms of apparatus involved to such an extent that it is sometimes difficult to remember that the treatment can be applied without any complicated apparatus at all. The authors are well aware of this, and they devote a most interesting part of their book to the four basic methods by which pyrexia can be produced in the living organism—namely, by inoculation with infectious diseases, by vaccine injections, by the introduction of specific chemical substances, such as proteins, into the body, and finally by physical means. Each method is discussed and evaluated, and it is the opinion of the authors that the most progress will be made in the future by the combined action of fever therapy with appropriate medication.

In the section dealing with physical methods three main categories are recognized. These are (1) by the applica-

tion of heat to the external surface of the body, (2) by enclosing the patient in a non-conducting covering, thus preventing any loss of heat, and (3) by the conversion of electrical energy into heat in the interior of the body. The technique and results for each of these methods are described, and this section of the book is illustrated by photographs of apparatus which give a general idea of the size and appearance of the different machines. The clinical results claimed for fever therapy in very diverse conditions are set out and commented upon by the authors. This book has much to recommend it as an impartial guide to the subject, and it is so well produced as regards printing and general arrangement that it is easy and pleasant to read.

EIGHTEENTH CENTURY SCIENCE

A History of Science, Technology and Philosophy in the Eighteenth Century. By Professor A. Wolf. (Pp. 814; 345 illustrations, including frontispiece. 25s. net.) London: George Allen and Unwin. 1938.

The second volume of the *History of Science* by Professor A. Wolf of the University of London proves that the encyclopaedist is not dead, but has only changed his character in accordance with the advance of knowledge. This second volume deals with the science, technology, and philosophy of the eighteenth century, and is embellished with 345 illustrations—portraits and apparatus. It shows how the work of the sixteenth century was developed in the succeeding hundred years, partly by the better education of the more learned classes, partly by a greater use of the experimental method of research, and partly by improvements in the working tools of science. Reliance on authority diminished, credulity lessened, and criticism became more acute. Credulity dies hard, for in the middle of the eighteenth century many who should have known better still maintained that Mary Toft of Godalming could produce rabbits instead of children. Knowledge was limited, for specialism was only beginning, and, as Professor Wolf's book shows, many were equally at home in more than one branch of science. Harvey, Boerhaave, Haller, and Réaumur were outstanding examples.

The method adopted by Professor Wolf is to give a short account of the advances made in each branch of science, to illustrate it by details of the life of the chief exponents, and to mention the writings by which they are best known. The sections of greatest interest to the medical reader are those dealing with zoology, anatomy, medicine, surgery, and morbid anatomy. Two small slips may be noted for future correction. Bonetus, the well-known author of the *Sepulchretum* and a founder of morbid anatomy as a science, is persistently called Boretus; the other is that Dr. William Hunter was a surgeon, whereas his whole interests lay in teaching and in midwifery. There is a short but good account of Jenner's work in connexion with vaccination. Professor Wolf shows that those who knew from personal experience the horrors of small-pox were wiser in their generation than those who have never seen a case. They rushed to be vaccinated; too many now avoid it. Anatomy, physiology, embryology, and morphology are grouped together under the general heading of "zoology," perhaps because the more general term "biology" had not been introduced in the eighteenth century. Trembley's experiments with polyps are duly recognized, but a line might have been added drawing attention to the beautiful little cuts which lend such great artistic value to his account of them. The question of spontaneous generation was under discussion throughout the period, but could not be finally settled until the advent of bacteriology more than a hundred years later.

For those who are interested in science generally, apart from medicine, there are admirable accounts of progress in mathematics mechanics, astronomy, astronomical and marine instruments, physics, meteorology, chemistry, geology, geography, botany, technology, psychology, the social sciences, and philosophy. There is a good index and the illustrations are admirable.

NEURO-ANATOMY

Textbook of Neuro-anatomy and the Sense Organs. By O. Larsell, Ph.D. (Pp. 343; 232 figures. 25s. net.) London: D. Appleton-Century Company. 1939.

A medical student requires in a textbook of neuro-anatomy a guide to the structural arrangements of the nervous system which will leave him neither floundering in an ocean of facts nor ignorant of the functional value of the various pathways and ganglionic masses. Consequently a successful text is one which states clearly the most important anatomical points in simple language, arranges these so that the student is able to build up his knowledge in an orderly manner, and uses important physiological facts and clinical findings in disease to stress the importance of the anatomical structures being considered. In Professor Larsell's textbook it is satisfactory to find these requirements fulfilled in large degree. At the end of each chapter references to modern neurological textbooks and articles are given, which permit the student to expand his knowledge as he wishes. The author has added sections on the special sense organs and their connexions with the central nervous system. These will be of great value, as so frequently the structure of the sense organs is considered apart from their neurological connexions. The volume is well illustrated.

SURGICAL PRACTICE IN HOT CLIMATES

Clinique Chirurgicale des Pays Chauds. By Botreau-Roussel. (Pp. 336; illustrated. 80 fr.) Paris: Masson et Cie. 1938.

This book on surgical practice in hot climates is a symposium under the general editorship of the Surgeon General who is the director of the Clinical School of the Health Services of the French Colonial Armies. The contributors are surgeons to the colonial hospitals. It is not purely a work on tropical diseases, but envisages the surgical aspects of diseases common in the French Colonial Empire. It is divided into sections contributed by different writers and has a number of illustrations, not all of which are good by our standards. In particular, those devoted to surgical technique seem to lack clarity.

The book is comprehensive and some sections are especially interesting, notably those devoted to a review of surgical tuberculosis in coloured subjects and to elephantiasis arabum. It is noteworthy that the author ascribes the latter disease to streptococcal infection, discarding the hypothesis of filarial origin, and sets out the reasons for his belief clearly and emphatica'ly. The section on liver abscess is valuable, though the illustrations are poor. There is a long study of yaws in all its aspects, while the surgical complications of malaria, dysentery, and abortus fever are well covered. The roundworm is usually regarded as outside the bounds of the surgeon, and it is most illuminating to read of the number of severe surgical conditions for which it can be responsible under suitable circumstances. The book is easy to read in the original French.

Notes on Books

Research and Statistical Methodology, Books and Reviews, 1933–1938, is a reprint from the *Mental Measurements Yearbook, 1938.* It is divided into two sections—research methodology and statistical methods—and each of these sections is subdivided. Medical readers will find the sections on education and psychology, medicine, medical and vital statistics, and social sciences of special interest. In every instance the title, publisher, and price (both American and English) are given. In many cases abstracts from reviews are added which indicate the outstanding features of the book. The volume concludes with an index of authors and titles. It should prove of value to research workers in all spheres. The publishers are the Rutgers University Press of New Brunswick, and the price is 1.25 dollars.

The work entitled *Cronologia, diferenciación, matrícula y distribución geográfica de las sociedades de ciencias medicas* (Córdoba, Rep. Arg.: Imprenta de la Universidad Nacional), by ENRIQUE SPARN, secretary of the Argentine Academy of Science at Córdoba and author of several previous works on the history of scientific societies, has been issued on the occasion of the sixth Argentine Congress of Medicine held at Córdoba last October. The book is divided into five parts. The first deals in chronological order with the foundation of the existing medical societies, the first of which, the Real Academia delle Scienze Mediche di Palermo, was founded in 1649, and the last, the Sociedad de Medicina y Cirugia del Paraguay, in 1935. The second part is concerned with the formation of societies devoted to special branches of medicine. Of these the oldest was the Académie Royale de Médecine de Paris, which later became the Société de Chirurgie de Paris. In the third part the societies are grouped according to the number of their members, the American Medical Association with 98,000 members holding the first place. The fourth part deals with the geographical distribution of the various societies, of which there are to-day at least 1,260 with 903,000 members. Europe possesses 827 societies, America 386 including 95 in South America, Asia 37, Africa 9, and Australia 1. In the last part special attention is given to the twenty-eight branches of medicine in the widest sense of the term. There are three appendices dealing respectively with medical societies with large libraries, of which the New York Academy of Medicine comes first and the Royal Society of Medicine second; the oldest organs of medical societies, of which the *Boston Medical and Surgical Journal* founded in 1790, now the *New England Journal of Medicine,* comes first; and the forty societies of human biology, of which the Société de Biologie de Paris is the oldest. The author is to be congratulated on this valuable work of reference, which requires very little knowledge of Spanish to appreciate, and should therefore be in the possession of every medical library throughout the world.

A fourth edition of Conybeare's *Textbook of Medicine* (E. and S. Livingstone, 1939, 21s.) has recently appeared. The main addition to this popular work is a section on psychological medicine, contributed by Dr. Desmond Curran and Dr. Eric Guttmann. An article on lymphogranuloma inguinale is now included in the section on venereal diseases, and several other chapters have been brought up to date.

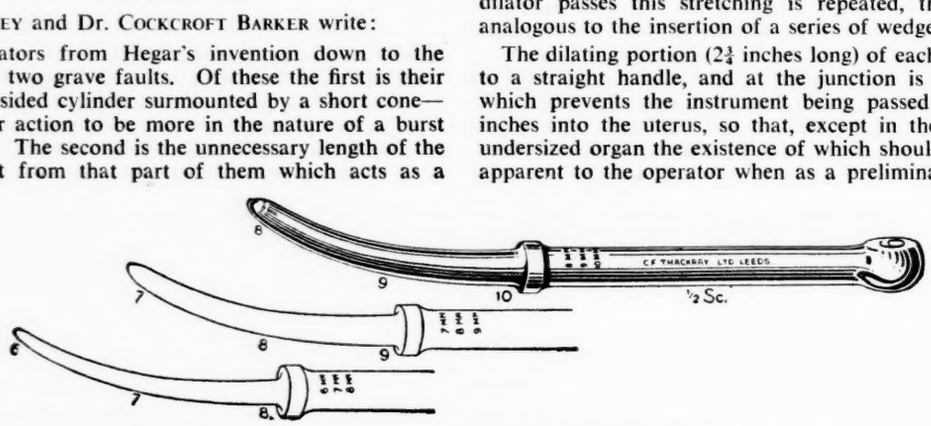
The seventh edition of WALLER and KAATZ'S *German-English Medical Dictionary* has recently been published (Leipzig and Vienna: Franz Deuticke, RM. 7, 1938). This is a small compact volume of 238 pages and is quite useful as a pocket dictionary. It does not appear to contain the number of archaic English words we noticed when reviewing the English–German part of this small work.

Preparations and Appliances

IMPROVED UTERINE DILATORS ON THE PRINCIPLE OF AN INTERMITTENT WEDGE

Mr. VICTOR BONNEY and Dr. COCKCROFT BARKER write:

All uterine dilators from Hegar's invention down to the present time have two grave faults. Of these the first is their shape—a parallel-sided cylinder surmounted by a short cone—which causes their action to be more in the nature of a burst than a dilatation. The second is the unnecessary length of the instruments, apart from that part of them which acts as a hold. It is to these two mistakes in design that the principal dangers of dilatation of the cervix are due—namely, rupture of the cervix and perforation of the body of the uterus.



The dilators Messrs. Thackray of Leeds have made for us avoid these faults. The dilating portion of each one is a much elongated cone regularly tapering from its base to its extremity. The diameter of the extremity of dilator No. 2 is that of the middle of the dilator No. 1. The diameter of the extremity of dilator No. 3 is that of the middle of dilator No. 2, and so on.

The result in use is a true wedge-like action. The first dilator having been passed and withdrawn, the second passes without resistance until it is half-way in, at which distance its half-way diameter exactly corresponds to the amount of dilatation that the base of the first dilator has effected. From then onwards until its complete insertion a true stretching force is exercised, and as the proximal half of each succeeding dilator passes this stretching is repeated, the process being analogous to the insertion of a series of wedges.

The dilating portion (2¾ inches long) of each dilator is joined to a straight handle, and at the junction is a projecting rim which prevents the instrument being passed further than 2¾ inches into the uterus, so that, except in the case of a very undersized organ the existence of which should be immediately apparent to the operator when as a preliminary he passes the sound, perforation of the uterus is impossible. The dilating portion is curved in the normal curve of the uterus.

These dilators have been in use at the British Postgraduate (Hammersmith) Hospital for the best part of a year now, and all are agreed that they are superior to the older-fashioned instruments, for they do not rupture the cervix as the latter so often do, and the sudden slipping in of the instruments with consequent perforation of the uterine wall is impossible, not only because of the projecting rim but because the wedge shape is incompatible with the sudden giving way of resistance; in short, they make the operation both easier and safer.

The dilators are made in solid rustless steel.

BRITISH MEDICAL JOURNAL
LONDON
SATURDAY APRIL 15 1939

THE LISTERIAN IDEA

The Lister Memorial Lecture by Professor René Leriche which we print this week (p. 759) is before everything else a protest against conservatism. Lister himself, perhaps more than any other great innovator in the history of medicine, had to contend with this attitude in its most self-complacent and bigoted form, and no tribute to his memory could be more appropriate than this plea for an open mind and willingness to try to improve even on present-day surgical practice. "Even a surgeon is a creature of habit." Applied to operative technique this sentence would disregard the magnificent spirit of enterprise which has so widely extended the scope of surgery in recent years: applied as it is to antiseptic practice, it could perhaps be improved by omitting the first word.

It is in no perfunctory spirit or by any stretch or twisting of the truth for the sake of scoring an appropriate point that Professor Leriche claims a place for the original Listerian idea in modern surgery. One would like to hope that his words will finally put an end to statements that asepsis and antisepsis are separate and somehow inconsistent ideas. There has never been any substance or sense in the supposed conflict between these two. Heat can be used to sterilize instruments, dressings, and gloves: this is asepsis. It obviously cannot be used to sterilize the patient's skin, the stump of his appendix, or the conjunctiva of a newly born infant; whatever measures are taken for these must be chemical, and this is antisepsis. Of course there are those who say that thorough washing and cleansing of the skin is more important than the use of any disinfectant, and that the prophylactic treatment of wounds by chemical means has little value, regardless in the latter connexion of numerous and careful experiments which show the contrary. It is precisely because there is so much of this contemptuous cynicism, mingled with some possibly misplaced enthusiasm and a great deal more of mere antiseptic automatism, that a fresh approach to this subject with a really open mind would be so valuable. There are certain established functions for the antiseptic method which are almost universally recognized, although the means taken to discharge them vary widely. The core of this controversy is the proper chemical treatment of a recent dirty wound, and not only practice but belief in this matter vary even more widely still. The best measures for this purpose can be deduced from theory and animal experiment: this has been done. They can finally be confirmed by clinical experiment, and ample material for this is to be found in any casualty department. It should not be difficult, and it would certainly be instructive, to conduct observations on a large scale in which the manner and rapidity of wound healing as well as the frequency of serious infection are studied after primary treatment with different antiseptics. Although certain acridine compounds have strong experimental claims to pre-eminence for the prophylactic treatment of wounds, these claims have yet to be confirmed by clinical study so planned as to carry conviction. This is a time when people would take kindly to such a project. The antiseptic idea, which was in some danger of dying from neglect and inanition, has been brought again to the forefront by two striking proofs of its validity. We now have two urinary antiseptics which will really and quickly sterilize the urinary tract, and various compounds which when taken by the mouth will cause the destruction of certain bacteria in infected tissues in any part of the body. It is true that the *modus operandi* of sulphonamide compounds is still a matter of dispute, but there is no question whatever that they confer antiseptic power on the blood, and a reasonable probability that they fulfil and embody Ehrlich's ideal of a "therapia sterilisans magna."

It is a penalty of enterprise and originality that they provoke criticism, and among Professor Leriche's many new ideas the treatment of infected areas with novocain or intra-arterial injections of mercurochrome is not likely to be accepted without question. Mercurochrome is so feeble an antiseptic that if the adoption of this route enables it to function it would certainly be worth while to try something more efficient administered in the same way. Perhaps his most original and valuable suggestion is that unsatisfactory scars, peritoneal adhesions, and venous thrombosis may be the consequences of minor wound infections which, because they are accompanied by little or no suppuration, pass unrecognized. Most of the bacteria responsible for these in themselves trivial infections probably come from the air, and although readers of this *Journal* will know of Hart's ultra-violet lamp for sterilizing the air of an operating theatre, it may be news to many of them that formaldehyde is used for this purpose by Gudin in Rio de Janeiro. This is indeed Lister's carbolic spray in a modern and more efficient form; the wheel has turned full circle and taken fifty years to do it. Here again is a subject which merits careful inquiry:

are the benefits to be gained by maintaining the air of the operating theatre bacteria-free commensurate with the trouble and expense which this involves? And if so, has the last word been said on the best means of doing it? Much less attention has been given to the chemical sterilization of air than to any other of the larger uses to which disinfectants can be applied. The necessary concentrations for effective action in air are not known as they are known for all disinfectants in water. It is not entirely unlikely that a wider study of this subject would reveal a new method of sterilizing air which is cheap and unobjectionable as well as effective. At all events, let us not merely rest on the accumulated laurels of the present century, but strive by such means as form the subject of this lecture to make surgery even safer than it is to-day.

THE ANATOMY OF EMOTION

The emotions have so often shifted ground in their historical wanderings about the human body—"now in the humours, now in the heart"—that doubt is natural as to the stability of their present seat in the diencephalon. To such doubt Professor K. S. Lashley[1] gives expected support in a closely reasoned and sceptical article. He accepts the evidence that there are in the thalamic region centres in which some at least of the patterns of expressive movement are integrated. Its expression, however, is not the unique and essential feature in emotion, *pace* the James-Lange theory. Besides these bodily activities in which emotion is manifested there are other phenomena: the peculiar subjective quality, which makes it impossible for us to talk about emotion in animals in the same way as we speak of it in ourselves, and the driving power, the urge to action, which is the core of modern psychopathological theory. What bodily substrate these have is still unknown. If the phenomena of emotion—subjective, motor, and dynamic—always appeared together there would be some ground for assuming that the neural site of its motor centres was that of the emotional process as a whole. But emotion can be pathologically dissociated: a man who is overwhelmed with grief may express his affect in forced laughter, and it is clear that the centres for expression cannot in such a case be contributing to the quality of emotion or be identified with the "centres" for the latter. The observations of Head and others which appeared to show that the thalamus adds some emotional character to sensations are also inconclusive; the changes reported in this limited group of sensations in the presence of thalamic disease are primarily changes in intensity, duration, and localization, rather than affective changes in the real sense. After Lashley's destructive analysis it must be conceded that efforts to track emotion into some anatomical lair have been misguided or illogical, even though some of them are ingenious and almost cartographically detailed. Thus Papez[2] ambitiously tries "to point out various anatomic structures and correlated physiologic symptoms which, taken as a whole, deal with the various phases of emotional dynamics, consciousness, and related functions. It is proposed that the hypothalamus, the anterior thalamic nuclei, the gyrus cinguli, the hippocampus and their interconnexions constitute a harmonious mechanism which may elaborate the functions of central emotion, as well as participate in emotional expression."

But even if such ambitious projects are still far from achievement much has been done in localizing the motor centres for emotion in the diencephalon. Ectors, Brookens, and Gerard,[3] for example, have lately been able to demonstrate by electrical stimulation of the lateral wall of the third ventricle in the hemi-decerebrate cat that an anterior hypothalamic nucleus gives responses suggestive of rage (facial expression, autonomic and respiratory concomitants), while those of fear can be got from the posterior region of the hypothalamus; somatic motor responses are obtained also by stimulation of the posterior region. Findings such as these are the bridge between study of the neural substrate of emotion—often mistakenly emphasized as anatomical—and physiological study of its somatic phenomena. A valuable contribution to the latter has been made by H. I. Schou.[4] He set out to measure some of the autonomic changes that accompany emotion in neurotic patients. Blushing, skin temperature, metabolic rate, insensible perspiration, gastric secretion, carbohydrate metabolism, and adrenaline content of the blood were the functions studied. Changes were demonstrated in all of these, except perhaps the last. The value of the observations lies in their having been obtained during mild emotional disturbance; there was no gross upheaval in these patients, but only the moderate affective change that could be produced by psychiatric discussion of their troubles. The insistence nowadays in psychopathology upon emotional conflicts and the claim that these play a larger part than was formerly suspected in the genesis of ostensibly physical disease make such findings as Dr. Schou's relevant to the whole question of psychosomatic relationships in medicine. They provide a more easily measurable basis for inferring the psychogenesis

[1] *Psychol. Rev.*, 1938, **45**, 42.

[2] *Arch. Neurol. Psychiat.*, Chicago, 1937, **38**, 725.
[3] Ibid., 1938, **39**, 789.
[4] *Acta Psychiat.*, Copenhagen, 1937, Suppl. 14.

of some somatic anomaly than would any statistical comparison of the personality and history of patients having this anomaly with those of a group whose illness was incontestably and wholly physiogenic. In the latter method, interesting and informative though its results can be, fallacies inherent in all history-taking (errors of memory, retrospective falsification, confusion of cause and effect) and others of a more obviously statistical nature may detract somewhat from the value of the inquiry. Until a particular disturbance of physiological function has been demonstrated to happen regularly when an emotional change is provoked in a human being, is it safe to suppose that when it occurs in disease it can necessarily be attributed to emotional conflict, whether this be thought of as conscious or not? It may be convenient to make this supposition so long as it is recognized merely as a working hypothesis, but there are valid objections to putting it forward as evidence for a wide incidence of psychogenic illness. On the other hand, investigations like those of Dr. Schou are of no service when the concomitants of particular emotions are in question: the immense subjective differences between one emotion and another are not reflected in these or similar studies. They do not yet correspond, for example, to the observation of Ectors and his collaborators that the hypothalamus is not a centre for a general and indiscriminate "ortho-sympathetic" discharge in emergencies, but that there are several centres giving qualitatively discriminate responses, in which somatic and autonomic effectors co-operate.

MORE PITUITARY HORMONES

The search for hormones continues with unabated vigour, and there seems to be no end to the discovery of new principles in the anterior pituitary. In a recent lecture[1] Professor Collip catalogued the effects of anterior pituitary extracts as follows: (1) stimulus to bodily growth; (2) thyrotropic; (3) gonadotropic; (4) adreno- or corticotropic; (5) mammary secretogogue or prolactin effect; (6) diabetogenic; (7) ketogenic (in normal animal); (8) increase in liver fat; (9) decrease in blood lipoids; (10) pancreatotropic effect (increased secretion of insulin); (11) lowering of respiratory quotient, with sharp rise in respiratory quotient, in normal animals; (12) increase of metabolism in thyroidectomized animal; (13) contra-insulin effect; (14) inhibition of adrenaline hyperglycaemia; (15) glycostatic and glycotropic effect (retention and increase of carbohydrate stores); (16) chromatophore and erythrophore expanding effect (probably from pars intermedia). Beyond these actions, which are described as following injections of extracts prepared in a variety of ways, there is a claim[2] that administration by mouth of acid extracts of anterior pituitary causes a polycythaemia in normal or hypophysectomized rats, demonstrable not only in the peripheral circulation but also in the bone marrow. The vagaries of blood counts in rats being well known, this claim may well await the confirmation of further work. The factor responsible for Nos. 11 and 12 in the above list has only recently been described by Professor Collip and O'Donovan,[3] and deserves particular notice. This is prepared from desiccated anterior lobes of ox pituitaries by alcohol precipitation of a concentrated aqueous extract made at *p*H 5 from glands extracted with acetone and then treated with aqueous alkali (*p*H 10) for four hours. This new principle injected into rabbits leads to a sharp rise in consumption of oxygen within a few hours; there is a fall in the respiratory quotient and some rise in production of CO_2 (the latter, of course, relatively less than the rise in oxygen). It could be shown quite convincingly that this action is quite distinct from that of the thyrotropic hormone, although the new extract is powerfully thyrotropic. The thyrotropic factor can, however, be destroyed in a short time at the temperature of boiling water, whereas the new factor is stable to boiling for even two and a half hours. In further work comprising the existence of this principle, Professor Collip, with L. W. Billingsley and D. K. O'Donovan,[4] shows that the metabolic effect does not take place through the mediation of the pituitary, thyroid, or suprarenal glands. The possible part played by the liver is to be investigated, but these workers believe that the specific metabolic principle acts directly on the peripheral tissues. The new substance appears to be present in almost any pituitary extract (pituitary colloid and extracts of pars intermedia both produce the metabolic rise), and is associated with a high content of melanophore hormone. The posterior pituitary substances—oxytocin and vasopressin—do not produce the metabolic effect. Careful analysis shows that the new principle can be differentiated from all pituitary hormones except the melanophore substance of the pars intermedia. Of great importance were the further observations that this extract (specific metabolic stimulant) also gives rise to the following: (*a*) ketonaemia in fasted rats; (*b*) increased ketonuria and glycosuria in depancreatized-hypophysectomized dogs (Houssay); (*c*) inhibition of insulin hypoglycaemia and adrenaline hyperglycaemia; (*d*) glycostatic and glycotropic effects. It is at this point that we meet the encouraging suggestion of Professor Collip's that all the actions of the new extract are probably due to one substance. In another paper Neufeld and Collip[4] refer to the latter's "attempt to simplify the whole question of the multiplicity of pituitary hormones." The evidence, admittedly incomplete, strongly suggests that the ketogenic, anti-insulin, anti-adrenaline, specific-metabolic-stimulating, chromatophore-expanding, and glycostatic effects are all due to one substance, which is very resistant to high temperatures over a wide range of *p*H, readily dialysable, and recoverable at the cathode in an electrodialysis cell. In a significant passage in the

[1] *Edinb. med. J.*, 1938, **45**, 782.
[2] *Presse méd.*, 1938, **46**, 1506.
[3] *Endocrinol.*, 1938, **23**, 718.
[4] Ibid., 1939, **24**, 63.
[4] Ibid., 1938, **23**, 735.

previously quoted lecture Professor Collip said that "in view of the fact that there are only three different cell types in the gland [anterior pituitary] it is *a priori* highly improbable that the number of individual hormone substances elaborated by the normal functioning gland can greatly exceed the number of cell types." With this statement, coming from so authoritative a source, we may well anticipate that a clarification of the anterior pituitary problem is not far ahead.

IS THERE A WAKING CENTRE?

The observation of independent sleep in a pair of Siamese twins who had a continuous vascular system has recently been reported in Russia. This provides an isolated but strong piece of evidence against the chemical theory of sleep, against any hypothetical "hypnotoxin" in the circulation. At the same time interesting observations on the neural mechanism have been made by Ranson,[1] who reports the results of symmetrical destructive lesions of the hypothalamus in monkeys. As an outcome of similar investigations over a number of years he and his colleagues are able (with the Horsley-Clarke stereotaxic instrument) to place electrodes in the basal grey matter of the brain with extraordinary accuracy. In ten out of eleven monkeys with small bilateral destructive lesions of the lateral hypothalamus, at the level of the caudal border of the mammillary bodies, a state of deep sleep persisted for many days after recovery from the anaesthetic. In twenty-five monkeys with lesions elsewhere—more anteriorly or in the midline in the hypothalamus, and also in the thalamus—a similar state of sleep was only transient and was explained by oedema round the lesion. The somnolence resembled normal sleep in that the animals could be roused, and it was not related to hypothermia, which will happen with similar lesions. In all cases in which prolonged somnolence occurred the mammillo-thalamic tract and the lateral hypothalamic nuclear masses were destroyed. As a result of stimulation and decerebration experiments Bard has shown that the hypothalamus is the centre for integration of the sympathetic and somatic reactions involved in emotional expression, and that its activation produces an excited animal. Ranson suggests that the active hypothalamus discharges not only downwards through the brain-stem, cord, and peripheral nervous system into the body, but also upwards into the thalamus and cerebral cortex. There are known paths to subserve such a function. It is therefore logical to postulate the presence in the lateral hypothalamus of a co-ordinating mechanism for the maintenance of consciousness, for which the thalamic connexions, among others, are essential. Ranson thinks that, in opposition to all other theories, he has destroyed a "waking centre" in his experiments. This waking centre, the argument runs, normally exerts an emotional drive upwards to the thalamus and cerebral cortex as well as downwards through the brain-stem to the periphery. The constant stimulation of this centre by incoming impulses maintains consciousness, while removal of stimuli through quiet and relaxation, or by the fatigue of receptors, results in sleep. This conception of a waking centre is thus not far removed from views put forward by Kleitman.[2] The apparent conflict with the observations of Hess,[3] who produced sleep in cats, may be explained by the slow alternating current that he used having depressed rather than stimulated the lateral hypothalamic nuclei. Serota[4] has reported temperature changes in the hypothalamus during sleep, and he attempts to show that these are unrelated to blood flow or to temperature changes elsewhere. As both the heated and cool thermojunctions which he used will respond to changes in cell metabolism and blood flow simultaneously, his results are difficult to interpret. A further recent approach to the study of normal sleep has been made with the electro-encephalograph. Loomis, Harvey, and Hobart[5] have divided the march from consciousness into deep sleep and back to consciousness into five stages, on the basis of changes in cortical potentials; and Blake, Gerrard, and Kleitman[6] have attempted to correlate these serial changes with the concept of a hypothalamic centre which integrates or inhibits the cortical response to afferent impulses. These observations, especially Ranson's conception of a waking centre, have simplified the approach to the problem of sleep, but we have still no idea of the fundamental changes taking place in this centre—changes which can induce normal sleep irrespective of the environmental conditions.

[1] *Arch. Neurol. Psychiat.*, 1939, **41**, 1.

OTITIC HYDROCEPHALUS

Otitic hydrocephalus is a rare condition associated with inflammatory disease of the middle ear and mastoid, and is usually diagnosed as cerebral abscess. Williams[7] points out that there is no active process of inflammation of the meninges. The main feature is a great excess of cerebrospinal fluid, which accumulates either in the subarachnoid space or in the ventricles. It is possible for ventricular hypertension to exist alone if oedema of the ependyma occludes either the foramina in the roof of the fourth ventricle or the aqueduct of Sylvius. If these foramina are patent the subarachnoid space of the spinal cord, which is relatively capacious, may act as an emergency reservoir when cerebrospinal fluid is expressed from the cranial subarachnoid space; this spinal subarachnoid space is traversed by many thin-walled, easily compressible spinal veins, and the pia and the arachnoid have some degree of elasticity. The pressure of the cerebrospinal fluid, which usually corresponds to the venous pressure, is maintained by the secretion of the choroid plexus. When the intracranial pressure is lowered to correspond to atmospheric pressure and kept at that level by continuous drainage the cerebrospinal fluid is formed not only at the choroid plexus but also by transudation through all the small capillary vessels of the central nervous system. A low osmotic pressure of the blood promotes the exudation of fluid from the blood into the subarachnoid space, so that the intravenous injection of hypotonic solutions pro-

[2] *Physiol. Rev.*, 1929, **9**, 624.
[3] *Arch. Psychiat.*, 1936, **104**, 548.
[4] *J. Neurol. Physiol.*, 1939, **2**, 42.
[5] Ibid., 1938, **1**, 413.
[6] Ibid., 1939, **2**, 48.
[7] *Ann. Otol. Rhinol. Laryngol.*, 1938, **47**, 670.

duces a prolonged rise in the pressure of the cerebrospinal fluid and a swelling of the brain substance. To prevent hydrocephalus it is necessary to raise the osmotic pressure of the blood, and patients with intracranial lesions must therefore be supplied with enough salt. The intravenous injection of a hypertonic solution decreases the volume and pressure of the cerebrospinal fluid, and patients in deep stupor may be restored to consciousness by such an injection. The so-called "haemato-encephalic barrier" has been studied by many investigators. Substances introduced into the general circulation do not necessarily reach the cerebrospinal fluid, but all substances injected into the cerebrospinal fluid are found sooner or later in the general circulation—that is, the epithelium of the choroid plexus acts as a biological membrane in one direction and as a permeable membrane in the other. The fact that the cerebrospinal fluid is a "biological filtrate" and not a secretion supports the belief that intracranial hypertension is caused by obstructive phenomena. Chemical and cytological examination of the cerebrospinal fluid is not particularly helpful in the diagnosis of cases of otitic hydrocephalus. When changes are present they resemble those found in low-grade forms of meningitis. As the diagnosis of otitic hydrocephalus is seldom made beforehand, however, the surgical treatment adopted is often that for a suspected cerebral abscess arising from the ear condition. The exploration for abscess is negative, but the opening and emptying of the distended subarachnoid space usually has a favourable result and the patient recovers.

SPECIES SANITATION OF FILARIASIS

The value of accurate knowledge in the control of disease is well illustrated in a recent article by M. O. T. Iyengar.[1] Working from 1931 to 1934 on the epidemiology of filariasis in Travancore, he found evidence of heavy human filarial infection. The microfilariae resembled those of *Wuchereria bancrofti* in that they showed nocturnal periodicity, but morphological examination revealed that there were two types—*Microfilaria bancrofti* and *Microfilaria malayi*. Although mixed areas occurred, the bancrofti infections were typically urban, whereas the malayi infections were typically rural. One of the most interesting features concerned the methods of transmission. Whereas the important transmitter of *W. bancrofti* was *Culex fatigans*, the malayi infections were for the most part carried by mosquitos of the genus *Mansonioides*. These two mosquitos have different breeding habits, a study of which indicates an economical method for the control of malayi. The eggs of *Mansonioides* are not deposited on the surface of water but on the submerged leaves of the water weed *Pistia stratiotes*. In due course the larvae are liberated and make their way to the roots of the *Pistia*, where, attaching their breathing siphons to the root tissues, they obtain their oxygen from the internal air cavities of the plant's root. Similarly, the respiratory trumpets of the pupae are modified for piercing and the pupae obtain their oxygen in the same manner as do the larvae. This knowledge was utilized in an experimental control, which showed that removal of *Pistia* produced a marked reduction in the incidence of *Mansonioides* and controlled the transmission of the infection. Reporting on this work, Sweet and Pillai[2] were able to announce that since 1935 no *Mansonioides* have been caught in these areas, and examination of the night blood specimens of eighty-seven children of 4 years or under in the centre of the area showed one to be infected, whereas examination of the blood of eighty-seven children of a similar age group living outside the area revealed infection in twenty-three. The discovery of this method of "species sanitation" against the vector of malayi demonstrates the value of studying precisely the specialized habits of parasites and their vectors so that control measures can be devised which are efficient and at the same time most economical.

[1] *Ind. med. Res. Mem.*, 1938, No. 30.

NUTRITION, AGRICULTURE, AND THE FAMILY DOCTOR

Our *Supplement* this week contains the text of a "Medical Testament on Nutrition and its Relation to Agriculture," drawn up by the Medical and Panel Committee representing some 600 doctors of the County Palatine of Chester. The drafting of this document arose from their belief that the National Insurance Act —designed for "the Prevention and Cure of Sickness"— had, after more than a quarter of a century of medical benefit, done nothing for the prevention, though much for the cure, of sickness. It had failed in its major object. This failure was, the committee felt, a fact that had to be faced. Under the inspiration of Dr. Lionel Picton, whose energy and public spirit are known far beyond the county of Cheshire, the document was drawn up in order to call public attention to a manifest defect in the system of national health insurance. It was submitted to an open meeting of doctors, farmers, and others held in the Town Hall, Crewe, on March 22, and was supported by speeches from Sir Robert McCarrison and Sir Albert Howard. In it the doctors of Cheshire record their experience that a large proportion of the illness they are called upon to treat is due to "a lifetime of wrong nutrition." They feel that half their work is wasted, since their patients are "so fed from the cradle, indeed before the cradle, that they are certain contributions to a C3 nation. . . . Against this the efforts of the doctor resemble those of Sisyphus." Many investigators "from Lind and Captain Cook to Hopkins and the Mellanbys" have helped to unlock the door of knowledge in regard to the relation of food to health and disease; but for them "the master-key which admits to the practical application of this knowledge as a whole has been supplied by Sir Robert McCarrison," whose "experiments afford convincing proof of the effects of food and guidance in the application of the knowledge acquired." The results of these experiments, some of which are set out in the Testament, have carried complete conviction to their minds, "especially as those of us who have been able to profit by their lesson have been amazed at the benefit

[2] *Ind. med. Gaz.*, 1937, **72**, 730.

conferred upon patients who have adopted the revised dietary to which that lesson points." It is not the purpose of the Testament to advocate a particular diet but to emphasize that food is the foundation of health, that it must be fresh from its source, little altered by preparation and complete; and that, in the case of foodstuffs based on agriculture, the natural cycle from animal and vegetable waste through soil and plant to animal and man must be complete, without the intervention of a chemical or substitution stage. Sir Albert Howard's work on the nutrition of plants has seemed to the framers of the document to constitute a natural link in this cycle and to indicate clearly that "the better manuring of the home land so as to bring an ample succession of fresh food crops to the tables of our people, the arrest of the present exhaustion of the soil, and the restoration and permanent maintenance of its fertility concern us very closely." Such, in brief, is the Medical Testament of the doctors of Cheshire, "given to all whom it may concern—and whom does it not concern?" We commend it to the serious notice of the profession and to those responsible for the public health.

CONVEYANCE OF TULARAEMIA

We commented before in these columns[1] on the occurrence of tularaemia in small outbreaks in widely different parts of the globe, and particularly upon one or two cases in Ireland with features related to that disease. More recent communications confirm the impression that tularaemia is by no means rare, and that without exact investigation occasional cases may be missed through confusion with anomalous forms of typhoid fever. The mode of infection is not always clear, and it seems that there may be cognate forms or varieties of the organism *B. tularense* not hitherto recognized yet capable of transmitting the infection. An account[2] has recently been given of the isolation and investigation of one which caused a heavy mortality among the animals in the laboratory of the Pasteur Institute in Tunis. The pathogenicity of the organism for rodents was established by inoculation, and by morphology, culture, and agglutination the close relation to, if not identity with, the typical organism was made plain. From the experiments the organism recovered apparently presents the same serological affinities as does *B. tularense* with the *melitensis* group. A survey[3] in 1937 of tularaemia in Turkey, where the disease has recently been made notifiable, showed there were thirty-five cases among civilians and eighty-five among soldiers, compared with a total of 150 cases in the previous year, when the disease was first recognized in European Turkey. A new centre of infection was discovered in a town in Asia Minor, the patient being a butcher. All the butchers in the town were serologically tested, and three out of the fifty-one gave positive results. A new and rather disturbing discovery was made in the course of the investigations in an infected area in Thrace—namely, that *B. tularense* was isolated as a result of injecting test animals with samples of water from a local stream. It is supposed that the water became infected by the excreta of rodents living in the banks of the stream. During this investigation a drop of an emulsion of *B. tularense* was inadvertently conveyed to the eye of a laboratory worker, who developed a typical oculo-glandular form of the disease four days later and was ill for four months. Another accidental infection is recorded from New Orleans,[4] where an assistant cut her thumb while performing a necropsy on a negro dead of presumed typhoid fever, which turned out to be pneumonic tularaemia. In Sweden in 1931[5] three persons caught tularaemia after cutting up a hare. The same thing happened in 1937, when three persons fell ill after cutting up a sick hare which was given as food to young foxes; but the foxes got off scot-free. Among the Swedish cases of 1937—115 in all—only occasionally was there any definite history of contact with rodents, game, or ground animals, and the question is raised as to how far insects may act as carriers of infection. Direct evidence on this point is wanting. It appears, however, that infection may spread first among the ground animals in a place where direct spread to man may occur but is rare, and then, as the number of infected animals increases, blood-sucking insects are infected and convey the infection to man. If this reasoning is correct it would explain most of the features of the three outbreaks recorded at intervals of three years that have occurred in Sweden since the disease was first recognized.

[1] *British Medical Journal*, 1937, **2**, 1231.
[2] *Bull. Off. int. Hyg. publ.*, 1938, **30**, 2224.
[3] Ibid., 2226.

WAR WOUNDS AND AIR RAID CASUALTIES

We publish in this issue at page 785 the first of a series of articles on the treatment of war wounds and air raid casualties. These articles formed the basis of lectures recently delivered at the British Postgraduate Medical School. At the moment of going to press it looks as though the decision to print these articles may be justified by events. The alarms and excursions of war have become such a familiar part of daily life that we run the risk of a complacent belief that the alarms will never be more than alarms and the excursions more than military exercises or marauding raids. "It can't happen here" expresses a deep-seated wish that every one hopes will be fulfilled. But the shadows cast are grey enough to discount facile optimism. The medical profession has shown praiseworthy alertness in preparing itself for service in a national emergency, and the articles that will now take the place of "Surgical Procedures in General Practice" will, it is hoped, forearm those already forewarned. When the "war" series finishes the articles on surgical procedures in general practice will be resumed.

We regret to announce the death at the age of 92 of Dr. George Wyndham Crowe, who was honorary local secretary for the Jubilee Meeting of the British Medical Association at Worcester in 1882, and was elected a vice-president of the Association on the occasion of the Centenary Meeting in 1932.

[4] *J. Lab. clin. Med.*, 1938, **24**, 34.
[5] *Bull. Off. int. Hyg. publ.*, 1938, **30**, 2230.

WAR WOUNDS AND AIR RAID CASUALTIES

The following is one of a series of lectures on the Treatment of War Wounds and Air Raid Casualties recently delivered at the British Postgraduate Medical School

THE TREATMENT OF WOUNDS IN WAR

BY

Colonel J. M. WEDDELL, F.R.C.S.

Experience gained in previous campaigns is by no means always reliable. We have to be prepared for new types of destructive missiles and different conditions of fighting, with the result that the technique of wound treatment may require considerable modification. For example, the mobile warfare in South Africa over sun-dried relatively sterile soil was very different from the stationary trench warfare in France and Flanders. In South Africa it was found that with the application of the first field dressing and sterilization of the surrounding skin the majority of the wounds healed by first intention. In France the fighting was mainly stationary and over cultivated and highly manured soil. The result was that the men in the trenches were soaked and plastered with grossly infected mud containing organisms of every variety, including the anaerobes of tetanus and gas gangrene. In South Africa the majority of wounds were inflicted by the high-velocity small-bore bullet of the Mauser type and relatively few by shell-fire. In the great war, wounds from high-explosive shells and shrapnel increased to an enormous extent, and wounds from missiles of relatively low velocity, such as trench mortars, bombs, and hand grenades, were added, with exceedingly severe results. The advances made in recent years in mechanization and the power of the air arm will present new problems in the way of medical organization and requirements.

In the field, wounds are inflicted by bayonet, sword, and lance; by high-velocity small-bore rifle and machine-gun bullets; by shrapnel and high-explosive shells; and by bombs, hand grenades, and trench mortars.

Effects of Missiles

Bayonet, sword, and lance wounds are relatively infrequent in modern warfare. Many soldiers are killed outright on the field, and few survive to come into the hands of the surgeons. The wounds may be of any nature, corresponding to the type of weapon employed.

Rifle and machine-gun bullets depend for their effect on the velocity of impact and also on instability. The modern pointed bullet is more unstable than the blunt-nosed Mauser type, and inflicts more severe damage on internal organs. That is to say, there are relatively fewer clean perforations of bone and viscera, and more damage is done to the internal organs in the way of laceration and disruption. The small-bore bullets have a fixed weight and shape, and a high initial velocity which is lost slowly. They have great power of penetration and relatively infrequently lodge in the body. They do not tend to carry clothing into the wound. Explosive exit wounds at short ranges are almost invariable. Multiple wounds are uncommon, except when inflicted by machine-gun bullets or when the same bullet traverses several parts of the body. The explosive does not affect the wound.

Shell fragments, shrapnel, etc., have no fixed weight or shape. They have a lower initial velocity, which is lost more rapidly. They are haphazard in action, often cause multiple injuries, and depend for effect less upon velocity than on the size and shape of the fragments. They have a lower power of penetration and often lodge in the body. They frequently carry clothing and gross dirt into the tissues. Explosive exit wounds are rare. The effect of the explosive is as destructive as the fragmentation (that is, blast).

Infectivity of Missiles

All missiles must be considered as infected. The high-velocity rifle and machine-gun bullets, while they do pass through dirty clothing and unwashed skin, do not tend to carry clothing into the wound, and the infection is, as a rule, more superficial and more amenable to surgical treatment. Shrapnel, shell fragments, and fragments of missiles of relatively low velocity (bombs, hand grenades, etc.) very often carry clothing and gross dirt into the wounds, and all such wounds must be regarded as heavily infected.

Infection of the wound track is a most important factor. Experience has shown that organisms are implanted into the wound track, but do not tend to invade the surrounding tissues for about twelve hours. It is obvious that the application of antiseptics cannot sterilize the tracks of deeply penetrating or perforating wounds. The only effective method of rendering such wounds sterile is by early and efficient surgery, and if this can be carried out as soon as possible, and certainly before the lapse of twelve hours, there is a good chance of eradicating the infection, and in a considerable number of cases the wounds can be closed by primary suture. It must be stressed that these primary sutures take considerable time, and must be done by an experienced surgeon under the best aseptic conditions: in some cases excision cannot be carried out completely for anatomical reasons. To close an inadequately excised wound is to subject the patient to a very grave risk to limb or even life. Primary suture is the ideal treatment.

In cases received later than twelve hours, or when satisfactory excision of the wound track cannot be carried out, the method of delayed primary suture is most useful. This consists in excision of the damaged skin edges and track as completely as possible, removal of any foreign bodies, and efficient haemostasis. The wound is left open and packed with gauze soaked in flavine or some mild antiseptic. If progress is favourable, such wounds can often be closed by secondary suture three or four days later. Incidentally, cases treated by this method stand transport quite well and can be evacuated as soon as they have recovered from operation. To give these operations the best chance of success it is essential to get the patients to the operating centre as rapidly and in as good condition as possible for operation. Therefore, the principles of early treatment are: the control of haemorrhage, the prevention of shock, and rapid evacuation.

In cases received late, when gross infection has already occurred, or in wounds which have broken down and in which frank suppuration is present, the Carrel-Dakin method of irrigation is of great value. This consists in

opening up the wound in all its recesses and pockets, removal of foreign bodies and dead tissues, and efficient haemostasis. The Carrel tubes are laid in the wound to reach all the surfaces, which are protected by a layer of gauze; the wound is lightly packed with gauze and instillation of Dakin's fluid is carried out two-hourly. In appropriate cases the wounds may be closed subsequently by secondary suture. This form of treatment also requires meticulous attention to detail, and must be preceded by efficient surgery. It is not drainage in the usual sense of the word: no dependent openings or counter-incisions are used, and the success depends on the hypochlorite solution gaining access to the whole wound surface. The instillations are carried out at short intervals (two hours), as the activity of the antiseptic tends to wear off.

Plaster-of-Paris.—Many good results have been reported from Spain from the immobilization of infected fractures and wounds of the limbs in plaster on the lines of the Winnett-Orr treatment for osteomyelitis. Immobilization is of very great value in the treatment of war wounds. I am of the opinion that this form of treatment should be reserved for the base hospitals and not be used in the early stages, when there is any possibility of an anaerobic infection.

At the Dressing Station

In the field rapid evacuation is contraindicated in certain cases. These are cases in which shock is likely to develop or be aggravated during transport. Operation is therefore advised at dressing stations:

1. To control haemorrhage.

2. For the removal of crushed and mangled extremities. This can be carried out satisfactorily with local anaesthesia and often without an anaesthetic at all, as the limbs are frequently in a condition of "local" shock and sensation is absent.

3. Suture of gaping or valvular pneumothorax. This is most useful in cases where there are large open wounds into the pleural cavity. The tissues of the thoracic wall down to and including the muscles can be drawn together with temporary sutures to close the opening. This gives great relief to the respiratory distress of these patients, and can quite well be done with local anaesthesia or even no anaesthetic at all. If it is not possible to suture such a wound, packing the cavity with a large pad of moist gauze and approximating the wound edges with strapping will also afford considerable relief.

The treatment of gunshot wounds involves first aid and operation. First aid is given to tide over the immediate necessities of the patients, to relieve their pain, and to put them in as good a condition as possible for operation. Operative treatment will be necessary for all the more severe types of wounds if time and conditions permit. The principles of first-aid treatment are: the control of haemorrhage; the application of a protective dressing to the wound and the immobilization and extension of fractures; and the prevention and treatment of wound shock.

Control of Haemorrhage

Tourniquets are not nearly so frequently necessary as might be imagined, and a tourniquet can be a very dangerous weapon. Every patient on whom a tourniquet has been applied must be clearly labelled with the time of application. The pressure needed is only equal to that of the blood pressure. The dangers are: (1) Local infection, which almost invariably follows the prolonged use of a tourniquet. Massive gangrene of limbs has occurred on many occasions. (2) Tissue damage and shock. (3) They are very painful. Tourniquets are more dangerous on the upper than on the lower extremity.

Fractures

The immobilization and the extension of fractures at the earliest possible moment are of the utmost importance. Immobilization during transport adds very greatly to the comfort of the patient and is of much assistance in the prevention of shock. Considerable swelling may appear in a wounded limb within a few hours, from either haemorrhage or inflammation. Such swelling occurring under a firmly applied splint may cause pressure effects varying from a local pressure sore to complete gangrene of an extremity. It should therefore be the rule (at a dressing station) to take down and examine any splinted limb in which there is excessive pain or in which the circulation of the extremities appears to be impeded.

Prevention and Treatment of Shock

Without going into the pathology of wound shock, there are several points in the treatment of the wounded which undoubtedly do prevent shock or assist in its treatment. These are:

Warmth.—It is essential to keep the patients warm, but not overheated, as this will cause excessive loss of fluid by sweating. A patient on a stretcher must be protected with a waterproof sheet, and blankets underneath as well as on the top. Artificial heat can be employed in the way of hot bottles, electric cradles, etc. Ambulances must be warmed.

Restoration of Fluid.—In gunshot wounds the patients are often suffering from a shock-haemorrhage complex. Therefore haemostasis and the restoration of fluid to the tissues are of vital importance. For those who can take fluid by the mouth, hot sweet tea is an excellent means of restoring fluid and warming the patient. Other means are intravenous infusions and the transfusion of blood.

Relief of Pain.—The injection of morphine ($\frac{1}{4}$ to $\frac{1}{2}$ grain) is an excellent sedative: whenever morphine has been given it is essential that a record should be made of the dose and time of injection, and this must accompany the patient.

Some Useful Points

As already mentioned, the immobilization and extension of fractures are of the utmost importance. Patients should be handled and moved gently and placed in as comfortable a position as possible—for example, flexion of knees in abdominal wounds. Unnecessary changing of dressings should be avoided.

The following are a few points in the early treatment of some special cases.

Wounds of the Spine.—Fractures and wounds of the spine should be transported in the prone position on a stretcher, with the exception of wounds and injuries to the cervical vertebrae. In these the patient should lie on his back with a stiff pillow or support under the neck to avoid flexion. The above will apply especially to cases with spinal injuries from falling masonry or from being buried in trenches. In wounds of the spinal cord with paraplegia no attempt should be made to pass a catheter: the bladder should be allowed to distend and overflow, or expression may be tried. The most satisfactory method of preventing infection of the urinary tract in these cases is by a suprapubic cystotomy, which should be done at the operating centre.

Wounds of the Jaws.—Danger of suffocation is most commonly due to loss of control of the tongue. In this condition posture is of vital importance, and the wounded man should be placed on the stretcher in the prone position with the head hanging downwards over the end of the stretcher. It is essential to keep the tongue forward in these cases, and many lives might in this way be saved

by stretcher-bearers and others. Correct posture will also tend to lessen haemorrhage by keeping the tongue in a forward position; but it may be necessary to plug wounds external to the mouth and to apply digital pressure.

Emergency tracheotomy in the dressing stations has been much employed by the Japanese surgeons in China. This enables the oral cavity to be thoroughly packed so as to check haemorrhage, and is spoken of as a "life-saving" measure. They have a special emergency tracheotomy appliance.

By the courtesy of the President I had the opportunity of discussing the following points at a Committee composed of four members of the Council of the Royal College of Surgeons.

The Thomas Splint.—In the Army we rely on the large-ringed Thomas splint for the evacuation of the majority of lower-limb fractures from the front line to the casualty clearing station. The splint is applied over the boot and clothing, and the ring is packed anteriorly and laterally with tow or wool to retain it against the tuber ischii. The fracture is supported with Gooch splinting or Cramer's wire: extension is obtained by means of a skewer through the waist of the boot or a clove-hitch round the ankle and tied to the distal extremity of the splint. The splint is suspended to a bar fitting over the stretcher. Many modifications of the Thomas splint have been suggested, such as collapsible splints, half-ringed splints, etc. It was agreed that for the evacuation of casualties in the early stages the original Thomas model was the most satisfactory. As one member of the Committee put it, "a Thomas splint altered is a Thomas splint spoiled." Cramer's wire splinting would form a useful support for arm fractures and fractures of the foot and ankle.

Skeletal Traction for Fractures.—It was agreed that internal traction by pins and wires was unsafe unless one was certain of first-class aseptic conditions—that is to say, skeletal traction is completely ruled out in dressing stations; and probably even in a casualty clearing station from which patients are evacuated by ambulance train it is safer to rely on skin traction.

Plaster-of-Paris.—In the form of supporting slabs this would be of great use in casualty clearing stations. The encircling of compound fractures with circular plaster bandages is dangerous in the early stages.

Blood Transfusion.—That extensive use would be made of blood transfusions, and that stored blood in refrigerators could be used in the dressing stations and forward area. That "drip transfusion" would be of great value in the casualty clearing stations.

Advanced operating units are not advised, as it is considered better to get the patients to the casualty clearing station, where they could be operated on and remain for some days and be nursed under better conditions. Patients do not stand evacuation well after serious operations.

Anaesthetics.—It was agreed that gas-and-oxygen is the most suitable anaesthetic for the serious cases, and should be reserved for these. This involves the use of heavy and cumbersome cylinders, the supply of which might be difficult. For many cases ether may be used, and should be regarded as the standard anaesthetic. The employment of the barbiturates for patients already in a condition of shock is definitely dangerous. This also applies to spinal anaesthesia.

Immunization against Tetanus

With regard to tetanus, steps are now being taken to protect the Regular Army by means of active immunization with tetanus toxoid. This is administered in two doses of 1 c.cm., given at six-week intervals, and affords a degree of immunity which, so far as can be deduced from animal experiments, is more than sufficient to prevent infection. Although definite information is not yet available it is believed that the immunity produced in this way is of long duration. As our knowledge of the subject is as yet somewhat incomplete, this measure is being regarded as an additional line of defence, and it is proposed to give immunized soldiers who have received potentially infected wounds a single dose of 3,000 units of tetanus antitoxin. The necessity for this procedure may be in some cases rendered imperative by absence of information regarding previous active immunization.

THE PSYCHOGENESIS OF SCHIZOPHRENIA

LONDON LECTURE BY PROFESSOR JUNG

Professor C. G. Jung of Zurich addressed a large gathering at the Royal Society of Medicine on April 4, his announced subject being the scope and limitations of the genetic factor in psychoses. Dr. H. Crichton-Miller, president of the Section of Psychiatry, who was in the chair, described Professor Jung as standing in psychology for the opposition bench. In England the critical attitude towards psycho-analysis had been very much helped by Jungian opinion. One could differ from him without parting from him, and that could not be said of all leaders of medical psychology.

Schizophrenia and Neuroses Compared

In opening his address Professor Jung recalled that it was just twenty years ago since he read a paper on this same subject of the genetic factor before the Royal Society of Medicine. At that time the late Professor W. McDougall, whose recent death they all deplored, presided. It was difficult to say anything new on the subject. During those twenty years his experience had increased and his views had matured, but he could not say that his standpoint had undergone any radical change. Psychogenesis was still a modern, even an ultra-modern, problem.

Concerning schizophrenia, it had been agreed that it had an essentially organic aetiology, although for a long time specific destruction of the brain matter could not be proved. Even now the question how schizophrenia could destroy brain cells was not satisfactorily answered, much less the more specific question how far primary disintegrations accounted for the symptomatology of schizophrenia. The great majority of the symptoms were of a secondary nature and chiefly due to psychical causes. There were certain noteworthy analogies between schizophrenia and the neurotic condition, but the fundamental difference turned upon the maintenance of the potential unity of the personality. In schizophrenia the "splitting" was not relative but absolute. The hysterical patient might suffer from a sort of persecution mania very similar to early paranoia, but the difference was that in the case of hysteria the delusion could be brought back under the control of consciousness, whereas it was impossible to do that in the case of early paranoia. The dissociation in schizophrenia was not only far more serious than in a neurosis, but very often it was irreversible. It was no longer fluid, but was more like something which had been broken into splinters. The complexes had become disjointed fragments. The split figures in the personality dissociation of schizophrenia no longer co-operated with the patient's consciousness, they were not "tactful," they tortured the ego in a hundred ways. They had liberated themselves from the control of the ego to such an extent that their original participation in the patient's mental make-up had vanished beyond recognition. Dissociation had been carried to a degree unheard of in the sphere of the neuroses. The very foundations of the personality were injured.

Disintegration of the Personality

In the psychogenesis of schizophrenia the primary symptom coincided with the condition which Pierre Janet formulated as "l'abaissement de la tension psychologique." Could this primary symptom be considered as an effect of psychological conflicts? It was not necessary to discuss at length whether the secondary symptoms owed their existence and specific form to psychological determinations or not. Their structure and derivation were in no way different from those of neurotic symptoms, with the significant exception that they exhibited all the characteristics of mental content no longer subordinated to the supreme control of the complete personality.

A neurosis was a relative dissociation, and there was an acute desire to re-establish the severed connexion, resulting in violent conflict. A neurosis might be said to be a state previous to the critical point of disruption, and schizophrenia to be a state beyond it. In neurosis the danger line was approached but not crossed. If it should transgress the line it would cease to be a neurosis. Was it certain that a neurosis never overstepped the danger line? He was reminded of the fate of the man who thought he could safely bark at the moon because he was conscious that what he was doing was absurd. A sudden panic might render the individual helpless and desperate, and then all the suppressed material welled up and drowned him. In forty years' experience he had seen quite a number of cases which developed a lasting psychosis out of a neurotic condition. It might be assumed that they really suffered from a latent psychosis concealed in the cloak of a neurosis; but what exactly was a latent psychosis? The existence of strange unconscious material proved nothing at all. Most suggestive parallels were found in the mythology and symbolism of primitive races. The possible future psychosis had nothing to do with the peculiar contents of the unconscious mind, but it had everything to do with determining whether the individual could stand a certain panic or the chronic strain of a psyche at war with itself. Very often it was merely a question of a little too much, the last drop poured into the vessel already full, or the spark that fell into the heap of gunpowder. It was understandable how people became panic-stricken and eventually demoralized under a chronic strain, also how, when the will-power weakened and the self-control became slack, some particularly unruly parts of the patient's psyche assumed a certain amount of autonomy.

Thus far schizophrenia did not behave in any way differently from an ordinary psychological disorder. The real trouble began with the disintegration of the personality and the divestment of the ego complex of its supremacy. He had stated that the primary symptom appeared to have no analogy with any kind of functional disturbance, but he had omitted to mention the phenomenon of the dream. Dreams could illustrate all stages of personal disintegration. It was no exaggeration to say that insanity was a dream which had replaced the normal consciousness. The phenomenology of the dream and of schizophrenia were almost identical, with the difference that the one state occurred normally under the condition of sleep, while the other absorbed the waking state.

The Analogy of Dreams

To explain the dream recourse must be made to historical or primitive symbology. In schizophrenia there was a mixture of personal and collective material* just as in dreams, though the collective material seemed to prevail in the schizophrenic state, while in dreams, except perhaps in certain decisive periods of life, such as puberty, the material was chiefly personal and not collective. Mere personal matters and worries accounted sufficiently in general for dreams, but in the case of collective dreams one was not so sure of one's ground. With their weird and archaic imagery it was impossible to trace them back to personal sources. No sooner did they begin these studies in Zurich than they felt the need for additional information regarding historical and ethnical symbology.

It was especially schizophrenia which yielded an immense harvest of collective symbology. The neuroses yielded far less. The fact that schizophrenia upset the foundations of personality accounted for the abundance of collective symbolism, because it was this latter material which constituted the basic structure of personality. From this point of view it might be concluded that the schizophrenic state, inasmuch as it yielded archaic material, had all the characteristics which primitive civilization attributed to the grossest magic. The ego was not primarily enfeebled; on the contrary it was the unconscious that was strengthened by the supposed presence of a demon. Primitive theory did not seek the reason for insanity in the primary weakness of the conscious, but rather in the strengthening of the unconscious.

Professor Jung said that he had seriously considered the possibility that in these cases more than a normal amount of primitive psychology remained intact and did not become adapted to modern conditions. It was natural that in such a state a considerable part of the psyche should not catch up with the normal progress of consciousness, so that in the course of years the distance between the conscious and the unconscious increased, producing at first a latent conflict, which presently became manifest when a particular effort at adaptation was needed, the primitive then breaking through with contents that were too incomprehensible and strange for assimilation to be possible. Such a moment marked the beginning of the psychosis in a great number of cases, and though many patients might yet be capable of producing a modern and sufficiently developed consciousness, such consciousness showed early signs of a self-defensive nature, a symptom of weakness and not of strength.

Precipitating Cause and Prognosis

The question still remained, Was the cause of schizophrenia psychological or not? It was well-nigh impossible to prove that schizophrenia was an organic disease, and equally impossible to show an exclusively psychological origin. It was well established that many cases developed out of emotional shock, disappointment, or reverse of fortune.

Many years ago a young student in Zurich was disappointed in love, and had a katatonic attack. He recovered after a few months, left the town, finished his studies, and became a highly successful professional man. Years afterwards he returned to Zurich, and immediately had another katatonic attack, was taken into the clinic, got well again, went away and resumed his work. Later he obeyed some impulse to revisit Zurich and the whole thing recurred.

An old teacher of his used to say that a psychological cause could only release symptoms of the disease, not the disease itself—a statement which might be profound or the reverse. At all events it illustrated the alienist's perplexity. It might be said that this patient returned to Zurich when he felt the attack coming on, but this he denied. He felt an impulse to come back to the place where he had had the early love experience. Who knew what ecstasies and adventures he enjoyed during his period of insanity? Professor Jung remembered a schizophrenic girl telling him that she hated him because he made it impossible for her to return to her "beautiful psychosis."

On the subject of prognosis, he said that this in schizophrenia was considered bad, but it was based on mental hospital figures, and these related to the worst cases. Numbers of such cases never came to hospital, their condition being camouflaged as phobias or hysterias. There were numbers of latent psychoses, and quite a number

* Professor Jung describes as the "collective unconscious" the primitive instinctive tendencies of the life of the race which may manifest themselves in dreams.

not particularly latent, which under otherwise favourable circumstances could be submitted to psychological analysis with, sometimes, quite good results. Even if he was not very hopeful about such a patient he tried to give him as much psychological treatment as he could stand, because he had seen a number of patients whose later attacks had had a less severe character and a better prognosis on account of increased psychological understanding. The results of occupation therapy in mental hospitals showed that the status of hopeless cases could be considerably improved.

For about fourteen years he had seen some fifteen times in the course of the year a woman now 64 years old. She was a schizophrenic, and had spent months in mental hospitals with acute psychosis. She suffered from numberless "voices" distributed all over her body. One of these "voices" he found fairly reasonable and helpful, and tried to cultivate it, with the result that for two years now the right side of the body had been free from voices, while the left side remained under the domination of the unconscious. Unfortunately the patient was not intelligent, and he could only establish a fairly good rapport with her by adopting the terminology of the early mediaeval ages, and speaking of devils and witchcraft, telling her that she was possessed by a devil, with which she quite agreed.

Professor Jung concluded with the remark that what was called medical psychology was a very one-sided affair. It might afford some knowledge of everyday complexes, but in ordinary application it did not range far enough. True medical psychology would embrace the history of civilization, of philosophy, of religion, and, quite particularly, of primitive mentality—a vast, almost unexplored area.

FIRST-AID POSTS IN LONDON

Proposals for the location of first-aid posts have now been submitted to the Ministry of Health by almost all the metropolitan boroughs, and these proposals have been agreed with the Department, either officially or provisionally, by the London Hospital Officer in eighteen of the boroughs and in the city. Eventually the county will have over 200 aid posts, excluding the number which are to be established by the Port of London Authority in the dock area.

Use of Hospitals

The borough councils have revised their earlier proposals in order to make as much use of medical premises as they could, with the result that in the schemes already approved more than half the posts will be in hospitals, clinics, or health centres. This is thought to be particularly important in London, where the public know the hospitals well and would instinctively make for them if injured in the streets. It has the further advantage that the posts could be ready to function at very short notice. It has been found that in London aid posts can be established almost entirely in existing buildings, but where there are no suitable premises a new building will have to be erected. Posts of the Nissen hut type are to be put up at Battersea and Hackney. Three of these huts together form a unit, one for cleansing and two for first aid.

Mobile Units

There is to be at least one mobile unit in every borough. It is to consist of a van filled with cupboards containing the full medical equipment of a fixed first-aid post. A doctor and, if possible, a trained nurse would travel inside the van and it would be driven either to the scene of a disaster—there to set up an aid post in the nearest convenient building—or to reinforce a fixed aid post where the casualties were very numerous. The possibility that in London streets might be blocked makes it impracticable to rely mainly on the mobile units, but it is thought that they should prove very useful on occasion in supplementing the fixed posts.

Underground Posts in Westminster

There are to be ten first-aid posts in the City of Westminster: one in the new Westminster Hospital, which, as a frame building of modern construction, is well adapted to withstand splinters and the effects of blast; one in the basement of Adelphi House (to serve the Charing Cross Hospital); one in the basement of Kingston House (to serve St. George's Hospital); and two more in the basements of Dolphin Square and Berkeley Square. The remaining five posts will be smaller and will all be located in hospitals: in St. Vincent Square, Soho Square, and three others still to be decided. The council is also considering the provision of a mobile unit.

It is proposed that sixty volunteers should be attached to each fixed first-aid post, working in three shifts of eight hours each. The personnel are almost all to be trained in first aid and will work under the charge of the doctor. Doctors are being allocated to the posts in peace-time and will be responsible for giving their own teams a certain amount of collective training. More volunteers are required.

Converting Commercial Vehicles into Ambulances

The Minister of Health has issued for the guidance of scheme-making local authorities under the Air Raid Precautions Act, 1937, drawings of a stretcher-carrying fitment for converting small commercial vans into ambulances, accompanied by an explanatory circular (No. 1794, H.M. Stationery Office, 2d.) which describes the measurements, etc., of vehicles suitable for the purpose. The fitment forms a unit designed to carry two stretchers, one above the other. It will generally be possible to place one unit on each side of the van, leaving a narrow gangway to enable the ambulance attendant to render emergency aid to injured persons in transit. The fitments can be readily and inexpensively made and quickly assembled, when necessary, without technical skill; the unassembled parts form a bundle of small compass for storage in peace-time. The Minister suggests that volunteer ambulance drivers and attendants should be given practice, as part of their training, in the simple operation of assembling the fitments, so that they may be able to assist with it at the onset of an emergency.

THE CANCER CAMPAIGN

Viscount Hailsham presided at the seventieth quarterly meeting of the Grand Council of the British Empire Cancer Campaign, held at 11, Grosvenor Crescent, London, on April 3.

The following grants were approved, making a total to date for the year 1939 of over £48,000: £1,000 to the North of England Branch of the Campaign for the continuation of the special short-wave investigations being carried out under the direction of Dr. F. Dickens, and £160 to Mr. E. Nevill Willmer, working at the University of Cambridge. A further sum of approximately £6,500 was allocated to meet the expenses to be incurred during the second year's working of the scheme of the Clinical Cancer Research Committee. This scheme covers an inquiry into all fresh cases of cancer in the London area, and during the first two years the results of investigations into approximately 34,000 cases will have been placed at the disposal of the committee.

A report was received regarding the joint discussions which had taken place between representatives of the Campaign, the Medical Research Council, and the National Radium Commission on the subject of the installation of a cyclotron in this country primarily for medical and biological investigations as affecting the cancer problem. The three bodies have now jointly agreed to refer this matter for further consideration to the Radiology Committee.

The application for affiliation from the Canadian Society for the Control of Cancer was acceded to. This organization

is sponsored by the Canadian Medical Association, and has been entrusted with the funds which were placed at the disposal of the trustees of the King George V Silver Jubilee Cancer Fund for Canada. Grand Council decided that the Campaign should send five delegates to the third International Cancer Congress, to be held in the United States of America in September, at which all the leading cancer research centres of this country will be represented. Dr. J. M. Woodburn Morison was invited to become the representative of the Campaign on the scientific advisory committee of the newly amalgamated Radium Institute and Mount Vernon Hospital.

TRAVELLING FELLOWSHIPS IN MEDICINE

The Medical Research Council invites applications for six Rockefeller Medical Fellowships for the academic year 1939–40. These are provided from a fund with which the Council has been entrusted by the Rockefeller Foundation of New York. They are intended for British graduates who have had some training in research work in clinical medicine or surgery, or in some other branch of medical science, and who are likely to profit by a period of work at a centre in the United States or elsewhere abroad before taking up positions for higher teaching or research in the United Kingdom. The stipend will ordinarily be at the rate of £360 per annum for a single Fellow, and of £480 per annum for a married Fellow. Travelling expenses and some other allowances will be paid in addition.

The Council also invites applications for four Dorothy Temple Cross Research Fellowships in Tuberculosis, which are awarded from a special endowment of which the Council is trustee. The object of these Fellowships, as defined in the trust deed, is to give special opportunities for study or research to suitably qualified British subjects of either sex "intending to devote themselves to the advancement by teaching or research of curative or preventive treatment of tuberculosis in all or any of its forms." The stipend will ordinarily be at the rate of £350 per annum for a single Fellow, and of £480 per annum for a married Fellow, with an allowance for travelling and incidental expenses.

Completed applications for Fellowships of either type must be lodged with the Council not later than May 8, 1939. Further particulars and forms of application are obtainable from the Secretary, Medical Research Council, 38, Old Queen Street, Westminster, S.W.1.

Various conferences on subjects of direct or indirect medical interest have been arranged to take place in Liége during the coming international exhibition there, which will continue from May to November. Cardiology is the theme for discussions on May 27 and 28, under the chairmanship of Professor L. Dautre-Bande. The University of Liége will conduct a series of courses on water purification on the first three days of June, and on June 10 and 11 the Belgian Society of Urology will hold its international reunion. On June 23, 24, and 25 there will be international discussions on social medicine. The reunion of the Belgian Society of Tropical Medicine will occupy September 23 and 24. The Belgian Society of Physiotherapy is to hold a national congress on this subject from July 17 to 22. Among the other medical events are the "Medical Days of Brussels" from June 24 to 28, the "International Days on Rheumatism" on the same days, with coincident meetings of the Royal Belgian Society of Gynaecology, the Society of Traumatology and Industrial Diseases, the Belgian Society of Ophthalmology, the General Association of the Dentists of Belgium, the International Union against Venereal Disease, and the international committee of the campaign against quackery. Further information can be obtained from the representatives in Great Britain of the Liége Exhibition—namely, the Leadlay News Service, Ltd., 9, Wine Office Court, Fleet Street, London, E.C.4.

Local News

SCOTLAND

The Nursing Services

An address entitled "Health and the Nurse," by Dr. A. S. M. Macgregor, medical officer of health for Glasgow, is reprinted from the *Proceedings of the Royal Philosophical Society of Glasgow*. Dr. Macgregor points out that the role of the trained nurse is no longer merely to tend the sick, for she now takes a prominent place, as teacher and social worker, in the defences against disease and in measures for the promotion of health. In Glasgow trained nurses have been employed to a large extent in the supervision of families transferred from slum areas to new houses, assisting them in the creation of a home in their new surroundings, and the experiment has proved highly satisfactory. Dr. Macgregor also reviews the position of domiciliary nursing, the hospital training of nurses, and the question of recruitment for the nursing service. The last-named problem is dependent upon conditions in industry. During prosperous periods it has been a matter of difficulty to find sufficient candidates of the right type, while, conversely, during times of depression and unemployment applications are numerous, a much freer selection is possible, and the standards of service and training correspondingly improve. Dr. Macgregor states that early in 1934 a decline became noticeable both in the quantity and in the quality of applicants, and it was found to be impossible to insist upon the educational standards previously required. The position in Glasgow, however, shows some improvement during the past year, possibly attributable to the announcement by the Government of its intention to set up a Departmental Committee on Nursing.

Royal (Dick) College Appeal

The governors of the Royal (Dick) Veterinary College, Edinburgh, have issued an appeal for funds to extend and provide additional facilities in the college. An original appeal launched by the late principal, Dr. O. Charnock Bradley, in 1936 for £40,000 has succeeded in raising about half of this sum, including contributions from various interested bodies and from the Government, which has subscribed £1 for every £1 given by the public. There is an immediate need for £6,000 to complete the main block of the extension now in progress. It is pointed out that the annual financial loss to producers of live stock and live-stock products in the United Kingdom amounts to £19,000,000, and that better provision for veterinary education in Scotland is one of the most effective ways of reducing this. An expert Government committee recently recommended for this college a total capital expenditure of £96,000, including the £40,000 scheme. Further laboratory and teaching accommodation is urgently required, as the number of students to be provided for has risen from under 150 in 1931 to 400 at the present time.

Aberdeen Medical Reports

At the 198th annual meeting of Aberdeen Royal Infirmary the Lord Provost presided, and the chairman of the board of directors said that the work of the infirmary was steadily increasing. During 1938 10,808 patients had been treated in the wards, as compared with 9,815 in the previous year. The daily average number of patients throughout the year had been 504, and on one occasion there had been 547. During the past year the radium department, which is a national radium centre, gave 545 treatments, compared with 504 in the previous year. The hope was expressed that with new facilities the public would seek advice earlier, and that the work of this department would be considerably expanded. The

ordinary income of the hospital had been £56,089, and the ordinary expenditure £72,383. At the same meeting the 138th annual report of the Aberdeen Royal Mental Hospital was submitted. The chief feature of this was a proposal for a reconstruction scheme at an estimated cost of £35,000. The reconstruction had begun in 1933, but a further improvement of the hospital buildings was contemplated, especially with the object of providing on the ground floor a dining and recreation hall to accommodate 450 patients, and for an occupational therapy department. In January, 1938, the University of Aberdeen had appointed a lecturer in psychopathology, and the Royal Infirmary had instituted a department of psychological medicine under his charge. This department included beds associated with one of the medical wards and an out-patient clinic at Woolmanhill and at Forresterhill on different days, with the object of treating functional nervous disorders and certain types of delinquency, and instructing medical students in the clinical aspects of this work. During the year 145 cases had been referred to the out-patient clinic and forty-seven treated as in-patients.

ENGLAND AND WALES

Sir Charles Hastings Lecture

The Sir Charles Hastings Lecture of the British Medical Association will be given by Professor V. H. Mottram, Professor of Physiology in the University of London, in the Great Hall of B.M.A. House, Tavistock Square, W.C.1, on Friday, April 28, at 8.15 p.m. Professor Mottram's subject will be "Nutrition and the Public Health." The chair will be taken by Dr. Robert Hutchison, President of the Royal College of Physicians. The lecture, which is given annually in honour of the founder of the Association, is open to the public. Members are invited to co-operate in making the lecture known to those of their patients who may be interested. Admission is by ticket, and application should be made as early as possible to the Secretary.

Domiciliary Midwifery Service in London

The first year's working of the domiciliary midwifery service in London has been reviewed by the London County Council. The staff of domiciliary midwives was originally fixed at forty-seven, but on April 1 it numbered fifty-seven. The number of confinements attended by the midwives during 1938 was, in the first six months, 1,558, and in the second six months 1,838. During the last nine months of the year 389 expectant mothers who had applied for admission to the Council's hospitals subsequently decided to avail themselves of the domiciliary midwifery service. The pressure of work is greatest in the Greenwich and Lewisham areas. Owing to the number of independent midwives who have intimated their intention to retire, and the many more who may be expected to retire before July 30, 1939, after which date they will no longer be able to claim compensation under the Midwives Act, 1936, it is proposed to increase the fixed establishment of domiciliary midwives from April 1 by twelve positions, bringing the number to sixty-nine, and from July 1 by six, bringing the number to seventy-five. The present cost of the service is £18,390 a year; with the new proposals the figure will ultimately reach £24,630.

Emergency Bed Service for Liverpool

Liverpool has given a lead to the North by establishing an emergency bed bureau which will operate on much the same lines as the service that was started in London last year. A central telephone exchange with private lines to the major hospitals in the city has been set up at the Royal Infirmary. Practitioners wishing to secure the admission of emergency cases to hospital will only have to telephone the central exchange, where up-to-date lists of available beds in the city's institutions will be kept and all arrangements will be completed. If the practitioner has a preference for any particular hospital an endeavour will be made to find a bed there for his patient. The bureau was opened on March 20 by the Lord Mayor of Liverpool.

SOUTH AFRICA

The Union Health Report

The annual report for the year ended June, 1938, of the Department of Public Health, Union of South Africa, stresses the backward state of sanitary and health conditions in most rural areas. It is stated that water supplies which are above suspicion are the exception even in the country towns, that housing is often extremely poor, and that sanitary services are frequently inadequate and in many cases altogether absent. Too often the smaller local authorities, while eager to install such amenities as electric light, are loth to incur expenditure upon essential health services. In these circumstances it is not surprising that the annual notifications of enteric fever have averaged more than four thousand during the past ten years. This disease appears to be becoming increasingly rural in distribution. During the year a large number of cases occurred along the course of the Berg River in Cape Province, and it was found that its head waters were being grossly contaminated through lack of sanitation in the villages and the washing of clothes in streams. Large numbers of campers, some of whom developed enteric, caused additional contamination. The report also describes milk-borne and fly-borne epidemics.

Considerable attention has been paid by the health authorities to the question of bilharziasis. An investigation in the Nelspruit and the White River districts of the Transvaal showed that 128 out of 144 non-European children were infected. The proportion of European children affected was considerably lower. A larger survey was carried out among native school children in Natal. Of 4,489 children examined 470, or more than 10 per cent., were diagnosed as suffering from active bilharziasis. A further 250 reported past haematuria, while ninety-six had blood in their faeces. The report stresses the significance of bilharziasis and intestinal parasites as factors in the incidence of ill-health and malnutrition in most native areas of Natal. With regard to plague, the number of outbreaks was only half that of the previous year, but the number of cases was greater. This was due to the bubonic outbreaks at Port Elizabeth, with twenty-three cases and sixteen deaths, and to a serious pneumonic outbreak on a farm near Dordrecht, where fourteen fatal cases occurred. The Port Elizabeth outbreak, the first for many years in an urban area, caused considerable concern to the authorities. The possibility of infection from shipping was ruled out, and it was assumed that infection was introduced through either the extension into the city of an epizootic among domestic rats from the neighbouring districts or by the transportation of infected rats from more distant foci inland in motor-cars, crated goods, or produce.

The list of State institutions, certified institutions, certified houses, and approved homes for mental defectives in England and Wales, 1938, which was incorporated in the twenty-fourth annual report of the Board of Control for 1937 (Part II), has been reprinted in pamphlet form, and copies may be obtained from H.M. Stationery Office, York House, Kingsway, W.C.2, price 9d. each.

Correspondence

Sacro-iliac Strain

SIR,—It seems a pity that so distinguished a surgeon as Mr. A. Lawrence Abel should have suffered "crippling agonies gradually becoming more and more unbearable" for two years, when there were so many orthopaedic surgeons around him any one of whom, I am sure, would have been only too willing to manipulate his back and cure him. However, it is satisfactory to learn that Mr. Abel eventually found a doctor in Cleveland, Ohio, who was able to perform the necessary manipulation—or was it the belt that cured him?

I suppose it is because Mr. Abel writes as a patient rather than from orthopaedic experience that his description of sacro-iliac strain (*Journal*, April 1, p. 683) is so completely at variance with everything that we have learnt about this condition during the last twenty-five years. Very simply the symptoms of sacro-iliac strain are pain and tenderness strictly localized to the back of the sacro-iliac joint. The pain is aggravated by sitting with the muscles relaxed. There may also be some pain referred to the outer side and front of the thigh. Flexion of the hip with the knee straight is somewhat restricted on the affected side, and when this movement is forced there is pain in the sacro-iliac joint. In cases of long standing a considerable degree of neurosis is often developed, and the symptoms of a "functional spine" are superimposed upon those of sacro-iliac strain. So far from sacro-iliac strain being the most frequent cause of sciatica, patients with uncomplicated sacro-iliac strain never have tenderness over the sciatic notch or pain referred down the course of the great sciatic nerve. Nearly all the symptoms described by Mr. Abel are typical of spinal arthritis with secondary sciatica, a condition which has nothing whatever to do with sacro-iliac strain. Ninety per cent. of cases of sacro-iliac strain can be cured by manipulation under an anaesthetic, followed by massage and exercises; those that are not so cured can be cured by arthrodesis.—I am, etc.,

London, W.1, April 3. A. S. BLUNDELL BANKART.

SIR,—I read with interest Mr. Lawrence Abel's clear and concise account of sacro-iliac strain (*Journal*, April 1, p. 683), and while I admire his detailed description of the differential diagnosis between lumbo-sacral strain, sacro-iliac strain, and lumbar back strain, I must confess that personally I have never found the signs he describes very helpful. The exact position of the painful spot, the angle of flexion of the hip, with the knee extended, which can be borne without pain, the presence or absence of pain on lateral bending of the lumbar area, the area to which pain is referred, and similar findings all sound anatomically so logical that one is inclined to attribute their source to midnight oil rather than clinical observation and trial. However, since Mr. Abel's treatment consists of empirical manipulation of *all* the joints of the lumbar spine and sacrum in *all* directions the differential diagnosis is of purely academic interest. May I suggest that careful palpation over the lumbar transverse processes often reveals tenderness and localized muscular tension on one side and over one or more transverse processes; such a positive finding is indicative of rotation of the lumbar vertebrae, which can sometimes be confirmed by *x*-ray examination. The treatment of such a condition, surely, should not consist of "wrenching" the spine in all directions, but of careful, gentle, though quick, movement of the involved joint or joints in one direction only. My own experience and that of my osteopathic colleagues has been that the more one practises manipulation the more specific one tries to become, and the fewer untoward reactions result from treatment.—I am, etc.,

London, W.1, April 3. W. HARGRAVE-WILSON.

SIR,—Mr. A. L. Abel has described in great detail the various symptoms associated with pain in the lower part of the back, and mentions two tests by which lesions of the sacro-iliac joint can be identified. Towards proving that the syndromes that he assembles under this title are indeed due to sacro-iliac strain we can discern in his article only two criteria: that the sacro-iliac joint is tender; that certain special tests are positive.

Taking these two points in turn: first, the sacro-iliac joint is inaccessible to direct palpation, being covered by one to one and a half inches of sacro-spinalis muscle. Any painful condition, therefore, of this muscle or its fascial covering will give rise to tenderness over the joint. Secondly, the two tests he employs are the elicitation of pain on straight-leg raising (Goldthwaite) and its variant where raising the leg on the unaffected side produces pain on the affected side. The former test will obviously be positive in any painful condition in the hamstrings, buttock, sciatic nerve, hip-joint, and lower back. Though it is true that, as Mr. Abel says, the latter test is "more" pathognomonic in so far as it does exclude painful conditions in the hamstrings, buttock, and sciatic nerve, it may be positive in any painful condition of the lower back, since at a certain point the test involves forward rotation of the pelvis with consequent movement of the lumbar spine.

Enough has been said, we think, to show that Mr. Abel's evidence for the existence of sacro-iliac strain does not bear critical inspection. We regard this diagnosis as a misnomer for a variety of conditions, of which the commoner are lesions of the sacro-spinalis and gluteal muscles and their fasciae, and of the interspinous ligaments. The evidence that we offer for this assertion is easily obtainable by the technique of diagnosis by selective local anaesthesia. When the infiltration of a structure by a local anaesthetic fully relieves the patient (for the duration of anaesthesia) we regard this as proof that the point anaesthetized was causing the pain. This method enables accurate diagnoses to be made with considerable certainty.

We should like to stress the point that the fact of relief having followed manipulation is no evidence for the patient's having had a sacro-iliac lesion, the more so as the manipulation Mr. Abel describes would seem to bear more on the lumbar spine than the sacro-iliac joint. However, we welcome Mr. Abel's paper in so far as it brings into prominence the fact that mobilization is excellent treatment for pain in the lower back. Nevertheless we feel that, though we have confined our criticism to points of nomenclature and the interpretation of signs, the paper taken as a whole invites contemplation of the principles on which all research must be founded if it is to constitute a basis for a real advance in knowledge.—We are, etc.,

ROBERT H. YOUNG, F.R.C.S.
London, W.1, April 5. JAMES H. CYRIAX, M.D.

SIR,—I have read with great interest Mr. A. L. Abel's communication on sacro-iliac strain. I consider, however, that the author has not sufficiently distinguished between sacro-iliac strain and sacro-iliac subluxation—a fault which

I may say is very common among writers on the subject. Personally I am of the opinion that a pure sacro-iliac strain is very rare indeed, as it is nearly always caused by a sacro-iliac subluxation. I must also point out that it is often combined with a lumbar malrotation, to which it is frequently secondary, though the reverse may be the case.

To treat a pure strain of the sacro-iliac joint uncomplicated by displacement or adhesions by means of mobilization in every direction does not appeal to me. And even if one were dealing with such a strain without displacement but with adhesions, I fail to see how mobilization could break down the said adhesions without inducing some movement of the ilium on the sacrum. If no displacement were present such mobilization might induce one. To propose to mobilize in every direction other joints of the body, say the shoulder or wrist, which were the seat of a pure strain without any displacement or adhesions would surely not be received with general approbation.

The success of Mr. Abel's methods must, I consider, be very largely due to the fact that what he regarded as sacro-iliac strains pure and simple were really sacro-iliac displacements, with or without concomitant lumbar malrotations. Had these been correctly diagnosed to commence with the manipulation advocated need not have been to mobilize in every direction, but to make attempts to move the displaced bone in one direction only—namely, the one which is diametrically opposite to the one in which the displacement had taken place. If the correct technique is employed the process is entirely free from pain and there is no need whatever for general anaesthesia. In over 2,000 cases of subluxated ilia that I have replaced during the last thirty years I have employed general anaesthesia in about half a dozen cases, and only then because the sacro-iliac subluxation was combined with lumbar displacements and marked lumbar adhesions. For details as to the technique of reposition I beg to refer to my original paper of 1934 (*Brit. J. phys. Med.*, **8**, 191).—I am, etc.,

London, W.1, April 6. EDGAR CYRIAX, M.D.

Sterilization of Dressings

SIR,—Before passing to the chief object of my letter I will answer a question your learned leader-writer puts in the important article on the sterilization of dressings and syringes in the *Journal* of April 8 (p. 731): "Are we driven to conclude that only a hospital which has all its sterilizing apparatus under the complete control of the bacteriologist is safe?" My answer is in the affirmative. I realize that your question and my answer apply with equal force to all institutions wherein dressings are used. Another point—the dry sterilization of rubber gloves and their powder does not bear bacteriological criticism so far as spore-bearing micro-organisms are concerned. The complacency of outlook upon the matters to which you allude and the general safety of surgical work are due to the facts that pathogenic spore-bearing micro-organisms are rare, and, as you say, "efficient laundry treatment yields a state approaching sterility."

The object of my letter is to draw attention to the ill-adapted contrivances called "drums." When they have been taken out of the sterilizer their lids and shut windows afford no protection against dust and germs and even more obvious living creatures. While still inside the sterilizer and after sterilization the contents of drums are exposed to infection from two sources. The open lids and windows of these drums expose their contents to the inefficiently protected air-inlet to allow the sterilizer to be opened, and the lids and windows must be closed, during which process further infection occurs. Some years ago the Genito-Urinary Company made drums for me that were shut at once dressings were inserted and not opened again until used, meanwhile having passed with complete efficiency the process of sterilization. These drums were too small and expensive for mass requirements. I used them in King's College Hospital. The fact that an efficient drum can be made proves that all dangers from this source can be avoided by future study.—I am, etc.,

London, S.W.1, April 8. G. LENTHAL CHEATLE.

SIR,—Your admirable leading article on sterilization of dressings and syringes makes very clear the difference in effectiveness between steam and hot-air sterilization. In this connexion there is one substance not mentioned in the article which is extremely likely to be a source of infection unless it is most carefully and thoughtfully sterilized, and which in practice is very often most indifferently treated. This is glove powder—talc—commonly known by the misnomer French chalk, which is generally placed together with the gloves in the sterilizer in the form of tampons or gauze packets, and subjected to ten or perhaps fifteen minutes at 15 lb. pressure.

This substance is a product of the earth, usually highly infected in the form in which it is generally received into the theatre, and which may very likely contain sporing organisms. Owing to its low conductivity of heat (it is a splendid insulator of heat) and its dense, closely lying character it is probably often hardly more than warmed throughout when placed in the sterilizer in the manner most usually adopted. Moreover, steam cannot penetrate through its mass easily, and not at all when the packets are placed in between the gloves, as often happens. If, as the article states, even streptococci may survive in the centre of bundles of dressings which may not have reached the desired temperature even after thirty minutes, how much more likely is it that such a non-conductive substance as talc may still contain living organisms of a very dangerous type, especially as, unlike towels and other dressings, it has not had the blessing of laundry treatment and is in any case almost sure to be originally infected.

An investigation of supposedly sterile glove powder would prove most surprising and interesting to many, and would lead to the separate sterilization of this substance at high temperature, even incineration, which does not in the least degree alter its usefulness, so as to ensure its freedom from spores.—I am, etc.,

D. T. DICKINSON.
Chester, April 10. Pharmacist.

Reaction to Whiff of Phosgene

SIR,—On March 27 a woman research student aged 32 sat for an examination in A.R.P., and in the oral was given phosgene to smell. The minute regulation phial had been broken in a bottle, and the student was the first candidate to smell it. She was speechless and coughing for about five minutes, but then recovered sufficiently to go home, and in spite of some coughing carried on as usual until the following afternoon—that is, about twenty-four hours from exposure to the gas. She was then out motoring, and began to have severe attacks of stridor and cough. A doctor who was with her got amyl nitrite capsules, which relieved the attacks somewhat. When seen in the evening she was a good colour and quite comfortable between the attacks, but these were so severe that I sent her into a

nursing home and ordered her oxygen and complete rest.

The next day Dr. A. G. Gibson saw her with me; her condition was much worse. During the attack the patient was pale, with a pulse of about 90, and in great distress. Adrenaline had no effect; only oxygen seemed to be of use. That night she was put into an oxygen tent, which very much relieved her condition, and omnopon, although failing to produce sleep, undoubtedly calmed the patient considerably. She is now (April 3) progressing steadily, though still unfit to leave the tent.

There has never been any fever; the pulse, except during the bad attacks, has been about 70 and the respirations 24 or 26. The stridor was inspiratory and at first very severe and noisy. On auscultation, apart from the stridor there were one or two rhonchi but never any sign of oedema of the lungs.

The interest of the case lies in the severity of the reaction to a very small dose of phosgene—a reaction so great that without oxygen it might have been fatal.

The advisability of using this gas in A.R.P. examinations might well be reconsidered.—I am, etc.,

Oxford, April 5. MARY RADFORD.

Emergency Treatment of Fractured Spine

SIR,—I read Mr. Geoffrey Hyman's paper in the *Journal* of March 4 (p. 444) advocating the use of the supine position in the emergency treatment of fractures of the dorso-lumbar vertebrae with great interest. I should like to point out, however, certain advantages in transporting cases of fractured spine by the method of the South African Red Cross. This method is used extensively on all the gold mines of the Witwatersrand, where fractured spine is a common result of truck accidents underground.

The treatment here differs according to whether the patient is found lying on his back or on his face. If found lying on his back, he is lifted in the same position by the "three-bearer straddle lift" (one bearer supporting the shoulders, one the trunk and buttocks, and one the legs) on to a stretcher prepared with a folded blanket arranged to support the fracture. If found lying on his face, he is put on the stretcher face downwards, with one blanket under the shoulders and one under the hips, thus maintaining hyper-extension. If found lying on his side, he is turned on to his face or back, whichever is the more convenient. It is an obvious advantage to avoid turning the patient over, in whatever position he is found lying, as this prevents any possible twisting movement of the spine.

With regard to lifting the patient on to the stretcher it is much easier to maintain hyperextension with the patient lying face downwards, as the middle bearer has a heavier weight to contend with than the two end bearers. When the patient is lifted face upwards the middle bearer has to exert more strength than the two end bearers, and unless great care is exercised the patient's trunk tends to sag in the middle, thus causing flexion of the spine.

In cases in which the patient is brought to hospital face downwards he can be transferred to bed in the same position and *x*-rayed with a portable machine in bed. He can then be slung up from gallows face downwards by the usual Böhler method and plaster-of-Paris applied in this position. He need not be turned on to his back at all until after the plaster has set, when he can be *x*-rayed with a more powerful machine through the plaster. Lateral radiographs with a portable machine are, of course, by no means perfect, but the outlines of the vertebral bodies can usually be made out and a compression fracture detected if present.—I am, etc.,

Boksburg, S. Africa, March 27. S. V. HUMPHRIES.

Classification of Breast Cancer

SIR,—The classification of breast cancer laid down by Mr. Ronald W. Raven in your issue of March 25 (p. 611) is very timely. At the present time there is a real need for the adoption of a uniform system of classification of breast cancer based upon clinico-pathological findings if further progress in the matter of prognosis and treatment is to continue. The present popular method of classification is based purely upon clinical findings, and thus it is not surprising if at times it proves unreliable as a guide to the exact state of affairs. One is familiar with the ulcerating type of scirrhus, which in some cases may exist for several years but in which the disease remains localized to the breast tissue. Such an instance would be curable by local amputation, although its grouping in Class III would suggest the possibility of inoperability. On the other hand, one sees the type of carcinoma which is represented by a small nodule, possessing only a doubtful attachment to the skin and no apparent glandular involvement (Class I), which subsequent developments have shown to be inoperable.

It will be impossible to avoid these irregularities of judgment so long as the clinical side of the disease only is taken into consideration, and therefore the sooner a clinico-pathological classification is brought into general use the better for all concerned. The combined method of classification has been employed for some time in connexion with carcinoma of the rectum, and its use allows an accurate assessment of the type and rapidity of growth and of the ultimate prognosis. At the same time, it has permitted the comparison of various methods of treatment on a scientific basis, and in this way has helped in the standardization and improvement of the treatment of that disease. Mr. Raven's classification is, as he emphasizes, essentially clinico-pathological; but it is also straightforward and complete, and does not involve the introduction of new terms or of examinations which are difficult to achieve. The general adoption of this type of classification would lead to a uniformity of examination in detail and scope which could hardly fail to produce a valuable result.—I am, etc.,

London, S.E.5, April 3. JOHN H. MULVANY, F.R.C.S.

Hormones for Undescended Testis

SIR,—Drs. A. W. Spence and E. F. Scowen (*Journal*, April 1, p. 693) would be in a stronger position to attack the surgical anatomy I teach if they understood what it actually is. Dr. Spence will remember that when I came to see his cases I told him that I thought all his very interesting work was vitiated by a fundamental anatomical error, which can only be really well demonstrated at an operation on an "emergent" testis. Had he accepted my invitations to attend such an operation he would not be talking about testes being adherent in the inguinal canal. Their coverings may be; the gland itself is always extremely mobile inside a serous sac. I do not quite see why it is so wrong to suggest that the inflammation seen in a human testis after the injection of female urinary extracts is the same process as the degeneration admittedly caused in the testes of experimental animals by the same procedure. Neither of us knows what this inflammation really means, but I would be very sorry to see it in a child of mine. In conclusion may I add:

1. I did not say that every child who has had these injections is rendered sterile; I sincerely hope that many of them are uninjured by it.

2. No one has asked to see any of the testes in which I suspect injury.

3. No one has commented on my remark that hormone treatment has no effect on the statistics of "undescended testis."

4. I have not raised the question of mental damage, on which a good deal of evidence is accumulating.

5. Dr. Spence, Dr. E. F. Scowen, or anyone else is welcome to attend an operation of mine or to invite me to attend any operation of theirs. If they can disprove what I teach I will acknowledge it publicly.

6. My object in reviving this correspondence is to get the profession in general to realize that nearly 80 per cent. of what they call "undescended testicles" need nothing more than to be left alone.

—I am, etc.,

London, W.1, April 3. DENIS BROWNE.

Procaine Infiltration for Sprains

SIR,—Flight Lieutenant E. J. Moynahan, in his article in the *Journal* of April 1 (p. 671), makes out a good case for Leriche's procaine infiltration treatment of sprained joints as shortening the period of incapacity. But it is questionable if Leriche's view of a sprain as being a nervous reflex causing vasomotor disturbance is correct. We cannot regard the syndrome of a sprain as being altogether due to vasodilatation. Most people, when they twist the ankle, experience at the time a wrenching sensation. The ligaments and fasciae are not torn right through but are lacerated to more or less degree—that is, various fibres or strands snap across at the point of greatest tension, and with them small blood vessels rupture when the fibres give way. It would require very sharp eyes to detect these torn fibres in exposing a sprain by operation as Leriche has done, and the usual discoloration cannot be attributed solely to vasodilatation.

The shortened period of Leriche's method can be equally well obtained by strapping the ankle with elastoplast outside of which a wet bandage is applied for the first day. This ambulant method allows the patient to walk about from the beginning, perhaps a bit tenderly at first, but soon moving easily when confidence in the ankle support is gained. And there is the great advantage that there are no "after-pains," which are noted in the article as being "often very great." Not everyone will agree with the nervous reflex theory: I prefer the more mechanical reason, and treat sprains on that basis. The chance of a chronic weak ankle from local anaesthetic treatment seems greater than when good strapping support is maintained while tissue reparation is taking place in a functioning joint.—I am, etc.,

Dalkeith, April 2. G. DOUGLAS GRAY.

SIR,—I read with great interest Flight Lieutenant E. J. Moynahan's article on the treatment of acute sprains by procaine infiltration.

Stimulated to try this form of treatment by Campbell's paper of last year in the *Lancet*, I have collected nineteen cases of severe sprain while casualty officer at the Huddersfield Royal Infirmary. I used 2 per cent. novocain, injecting varying amounts, up to 20 c.cm., into the haematomata around the ankle-joint. Two men returned to work the next day, and all the patients walked away without pain immediately after treatment. In only one case was there pain after one week, and I strapped this ankle. I have not noted the "after-pain." I found that my patients complained of tightness in the ankle. I have not been able to follow up the cases as I should have wished, but a few of them have reported satisfied with the condition of their ankles.

During the same period I have treated twelve fractures of the lower end, and one fracture of the upper quarter, of the fibula by injection. I injected 2 per cent. novocain between the fragments and also in the haematomata. All these cases were completely successful and there was no pain on walking. It was possible, however, to elicit pain by direct pressure over the fracture in some cases after two weeks. *X*-ray appearances give the impression that more callus is formed than in fractures treated by artificial supports.

I am satisfied that this treatment is of great benefit to the patient, perhaps in particular to workmen and to elderly women, in whom it is very gratifying to be able to send them away without strapping or plaster.

I am indebted to Mr. Barclay, honorary orthopaedic surgeon to the hospital, for his kind permission to write this letter, which I hope may be of interest.—I am, etc.,

G. ARROTT FOWLER, M.A.,

Leeds, April 4. M.B., B.Chir.

Sulphonamide Rash

SIR,—I was greatly interested in Captain G. W. Palmer and Captain D. R. Hanbury's communication published in your issue of March 18 (p. 561) as I have recently had a similar experience of my own. I had an attack of influenza and treated myself with proseptasine, 3 grammes the first day and 1.5 grammes the second day. This produced sickness and vomiting, as it had done six months ago when I had a streptococcal sore throat. I therefore discontinued taking the drug. My temperature still being raised the next day, I took three tablets of M & B 693, but had to stop taking it for the same reason. Two days later I developed a maculo-papular rash on the back and on the extensor surfaces of the arms and thighs. There was no scaling or irritation, and the rash faded after two days. I should like to add that after discontinuing M & B 693 I took a few aspirins, but I do not think that they could have been responsible for the rash, which I firmly believe was due to the sulphanilamides.—I am, etc.,

East Ham, March 3. G. S. WOLFF.

Double Function of Ocular Muscles

SIR,—Dr. F. W. Edridge-Green (*Journal*, February 4, p. 245) concludes that the extra-movement of the after-images described in his experiment results from displacement of the photochemical fluid brought about by pressure of the extra-ocular muscles upon the globe during rotation. By this I presume it is meant that the film of visual purple carrying upon itself a pattern of the optical image in the form of a chemically depleted area is impelled bodily beyond the limit of rotation by the intra-ocular pressure which, it is taken, attends contraction of the extra-ocular muscles upon the globe; and that the passage of this chemically depleted area across the retinal elements induces the excitation which leads to the appearance of persistence and movement in the original stimulus. It is not easy to agree with this conclusion in view of the generally accepted fact that the excitation persists in the rods and cones rather than in the visual purple, apart from the fact that the above description is more suited to explain the appearance of extra-movement in the *negative* after-image—which movement incidentally does not appear to take place at all.

Hydraulic pressure cannot be regarded as a factor of any great consequence in experiments of this nature, having regard to the high degree of mechanical perfection and the exquisite ease with which rotation of the globe is effected. Such features as (1) the spherical shape of the

eyeball; (2) its highly polished surface; (3) the soft, resilient, and greasy nature of the cushion upon which it rotates; (4) the peculiar structure of the sclerotic, so excellently designed to resist stretch; (5) the generous reserve in muscle power; (6) its smooth and delicate control; (7) muscle tonus; and (8) the action of the check ligaments all combine to ensure that the fluids in the eyeball will be disturbed as little as possible during rotation. If this were not so one might expect very frequent (but momentary) interruptions in vision as a result of interference with circulation in the retinal blood vessels. Momentum or centrifugal force offers a no more likely explanation, for, in addition to the above features, the impedance offered by this microscopic layer, closely sandwiched between two relatively stationary membranes and impaled, as it is, upon the rods and cones, must be very high indeed. Further, the oblique insertion of the muscles, their broad grip upon the surface, and the position of the origins ensure the proper distribution and direction of the strain upon the *sclera* and obviate the possibility of distortion of the globe or its contents.

In any case, it can quite easily be demonstrated that pressure or strain upon the *sclera* produces no movement of any sort upon an after-image. If one looks at a bright light for a moment or two and then closes the eyes a single after-image will be seen. If now the finger be placed upon the outer canthus of one eye and a fair pressure be applied to the globe it will be seen that one single after-image still remains although such pressure is ordinarily sufficient to produce diplopia. It would therefore appear that the position of the after-image is not altered by intra- or extra-ocular pressure and that *it is quite independent of the direction of the physiological axis of the globe.*

The application of severe pressure upon the globe may result in "spreading" of the *negative* after-image—the behaviour is much the same as that of a drop of coloured fluid between two slides under varying degrees of pressure—but no change will be seen in its position. The *positive* after-image shows no response whatever to such treatment. This points to the conclusion that the negative after-image may be due to depletion of the visual purple, and the positive after-image to psychological persistence or "shock." I have done some work with after-images and I am inclined to the theory that the extra-movement seen in after-images is due to the innervation to the extra-ocular muscles. There is much I would have liked to write about this, but I have already exhausted the space allowed to me.—I am, etc.,

Belfast, March 29. H. ELLIMAN.

Pruritus Ani

SIR,—I read with interest the article on pruritus ani by a distinguished surgeon, Mr. Lawrence Abel, in the *Journal* of March 25 (p. 627). With much of it I agreed, but I venture to offer a few comments from the point of view of a dermatologist. Rightly he lists psychological factors as a cause "in a very large majority" of cases, yet he does not suggest that any attempt should be made to tackle this side, merely saying "for obvious reasons this type of case is most difficult to treat." Worse still, in his paragraph on subcutaneous injections he refers to "the so-called functional type of case" in which "local applications to the surface have little or no beneficial effect," and implies that local applications a centimetre or two below the surface will give a satisfactory result. Actually if one devotes a little time to these cases, employing not professional psychology but sympathetic common sense, it is amazing how frequently one is able to discover some mental cause, possibly financial worry or marital disharmony, etc., which is clearly related to the pruritus and with which we must deal if we are going to treat our patients properly.

I agree that maceration and moisture around the anus are aggravating factors, and therefore am surprised that most of the applications suggested are of an oily or greasy nature, though of course these are sometimes necessary at first when there is much eczematization. Actually a calomel dusting powder alone, as advised by Thaysen, is one of the most useful local applications.

Finally, to a dermatologist it seems an extraordinary disproportion that three paragraphs are devoted to injecting piles, etc., and half a column to the injection of local anaesthetic, while only three and a half lines are given to a very tentative note about *x* rays. Assuming that no gross lesion is present, such as glycosuria or carcinoma, *x* rays are the most potent single weapon in the treatment of pruritus ani. They should be given in a course of three or four exposures of 150 *r* at intervals of three weeks, and not in a single small dose as the article implies.—I am, etc.,

Leeds, March 31. F. F. HELLIER.

Tuberculosis in Wales

SIR,—The publication of the report on the tuberculosis problem in Wales with its pointed reference to the failure of our medical efforts to control the spread of the disease must make the profession realize that something is radically wrong. The report draws special attention to such things as bad housing conditions and lack of fresh air, sunshine, and good food in those areas where tuberculosis was found to be most prevalent. These matters call for the strongest protest, and no effort should be spared by the profession in pressing on Parliament the need for immediate measures for dealing with the dreadful state of affairs found to exist. This report shows that in spite of our medical and surgical methods of treatment very few cases of tuberculosis are ever cured, and that our present methods for its prevention have utterly failed. Modern treatment does tend to prolong the life of the tuberculosis patient, but we find no evidence that all those patients who have received treatment are no longer a danger to the public, and it is probably true that most of our treated patients continue to spread tuberculosis till they die. This report makes us wonder whether we are really doing all we can to control the spread of tuberculosis. That great Scottish authority, the late Sir Robert Philip, for years urged the profession to alter the methods for controlling the spread of infection, and drew attention especially to the importance of protecting children from the risk of infection. In one of his lectures he stated:

"Tuberculosis infection, when once contracted, is fraught with consequences of supreme importance, directly to the individual child and indirectly to the community. . . . The earliest indications of infection are passed over as unimportant, and little is done until an alarm signal is given by grosser evidence of disease . . . so the dreary procession goes on from infancy, through childhood, to puberty, to adolescence, and to manhood. . . . And chiefly because, purblind, we have failed to see or failed to act. . . . Are we to wait till the child is obviously ill? Are we, in other words, to continue the feckless purposeless attitude that has prevailed in the past? I hope not. Surely it is time to realize that the guiding thread towards successful protection from these baneful consequences is within our grasp. . . . Whatever may be said as to the value of tuberculin in other spheres, there should be but one opinion as to its supreme value in the determination at the earliest possible moment of the occurrence of tuberculosis infection in the child. I am amazed that its employment is not demanded as a routine proceeding in the case of every child. . . . The test—repeated at intervals—is harmless and

painless; the knowledge obtained thereby is priceless. . . . Fully convinced as I am that knowledge obtained by the systematic use of the tuberculin test would effect the exclusion or limitation of an incalculable amount of disease and suffering, and make for a general elevation of health, I have again and again urged its general adoption by medical practitioners and health officers. I am satisfied that if the facts were known parents and guardians of infants would insist on the practice. . . . It is fortunately true that not every reaction calls for special care. It is not easy to single out those who will develop disease. . . . The knowledge afforded by a positive reaction at a given date will prove of supreme service in the interpretation of untoward symptoms and the adoption of appropriate precautions."

Surely it is time to consider seriously whether the advice of such a great authority should not now be taken, when we hear of the failure of our present methods for preventing the spread of tuberculosis. Sir Robert advocated the use of a liniment or ointment containing tuberculin, and claimed that by these means he had protected children from growing up tuberculous. A Dr. Gelpke urged the use of an anti-tuberculous blood plasma precipitate which could be given by the mouth for the prevention of tuberculosis. An appeal of mine for the use of this immune blood was made in the *Medical Officer* of May 4, 1935. Surely this advice given by Sir Robert Philip should be at once adopted and his method of treating these children, when necessary, be tried.—I am, etc.,

London, W.1, March 30. SYDNEY GORDON TIPPETT.

Research in Clinical Medicine

SIR,—Your leader on this subject in your issue of February 18 (p. 339) raises a question of supreme national importance. But it interests me more particularly as being one with which the universities are or should be primarily concerned, and, indeed, you cite London University as having undertaken in its professorial clinical units some provision for clinical research. The effort, as you rightly point out, has been largely thwarted by the burden of teaching which has overlaid the original motive for their establishment. Your leader was followed up on March 4 by a caustic but not unfair comment on the current position of the clinical professorial chairs in London University in a letter signed "Academicus," and my present purpose is to reply to the criticisms made in your leader and in your contributor's letter.

The judgment that English medicine tends to stress clinical rather than experimental work and is consequently left behind in the international race to advance medical science is not new and is probably just. I recall an interesting conversation with an emissary of the Rockefeller Foundation who was visiting London some four years after the end of the war. This gentleman was charged with distributing largess from the Foundation for medical education in Europe, and some medical members of the Senate, of whom I was one, made an effort to interest him in helping postgraduate medical education in London, then hopelessly neglected. His response was unfavourable, and one of his comments impressed me. He said that he had had personal opportunities during the war of observing the medical services in the Allied armies as well as in his own, and he had formed the opinion that while the British Army medical officer was an admirable practical man he was outclassed when the task was to investigate origins of disease—"*rerum cognoscere causas.*"

To understand the present position of the professorial clinical units in London University recognition of their historical origin is essential. They derive directly from the Report of the Royal Commission on University Education in London (1909–13). That report is deeply tinged with the predilections of its masterful chairman, Lord Haldane, then at the height of his intellectual powers and political influence. Haldane had himself studied at German universities, and the "German organization" (so styled in the report) of medical teaching was strongly favoured by the Commission. "The essence of this organization," the report declared, was that "the clinical teachers in a university medical school must be university professors in the same sense as the teachers of the ancillary medical sciences like anatomy and physiology." The principal obstacle to this consummation the Commission found lay in the existing medical schools. There were too many; they were too independent of the University of which they were supposed to form part, and were indeed "almost as closely connected with Oxford and Cambridge and the Conjoint Board as with London University."

The remedy proposed by the Commission was to select as centres for university teaching on the German model three of the existing medical schools, which would be required to submit to complete educational and financial control by the University; at these schools *all* the teachers should be of professorial type and status, and only students reading for university degrees (that is, essentially of London, Oxford, or Cambridge) should be admitted. The Haldane Report, published in 1913, lay dormant during the war years.

In 1918 Sir George Newman, the Medical Assessor to the Universities Branch of the Board of Education, which was then the source of Exchequer grants to the medical schools, submitted to Mr. Fisher, the President of the Board of Education, proposals (based on the Haldane recommendations) for the partial reorganization of medical education in London, which formed the basis on which the subsequent arrangements were made. None of the existing schools was willing wholly to surrender its independence as had been required by the Haldane Report, and the compromise arrived at was that at such schools as could provide an adequate number of beds (estimated at fifty to one hundred) to be allotted to the use of the professor and his staff, "units," at first restricted to medicine, surgery, and obstetrics, might be established, their cost to be defrayed by a special grant made for this specific purpose directly to the individual school. The professor would be appointed by, but paid independently of, the University.

The duties of the professor and his staff were to be (1) treatment of patients, (2) investigation of disease, (3) teaching of students, which together with research should occupy the greater part of the professor's time. The "unit" was to be run side by side with the existing "firms" of physicians and surgeons on the prevailing English system, and it was hoped and presumed that the adequate payment proposed for the professor would, by releasing him from the claims of private practice, supply the leisure necessary to prosecute research. By 1921 units thus constituted in medicine and surgery had been established at St. Bartholomew's, the London, St. Mary's, St. Thomas's, and University College Hospitals, and one unit in gynaecology at the School of Medicine for Women—eleven in all.

Pari passu with this development a fundamental change was taking place in medical education, conditioned by the growth of specialism, which necessitated the addition to the basic medical curriculum laid down by statute (that is, training in medicine, surgery, and midwifery) of "instruction" in a multitude of special subjects set out first in full detail in the revision of the curriculum undertaken by the General Medical Council in 1923. The period of professional study (five years) previously exacted by the revision of 1893 remained unchanged, so that the

new requirements had somehow to be crowded into the same period of study. The burden thus laid upon the teachers, especially in the final subjects, explains, in my submission, the present development which your leader deplores—namely, that teaching has swamped and largely ousted research even in the units, which have in fact been obliged to shoulder the main responsibility for teaching at the schools where they have been installed. The memorable "Adami Report" of 1924, commenting upon the position after three years' experience of units, while deprecating the curtailment already obvious of free time for research, declared that, "so great is the improvement brought about in the teaching of the student and coincidently in the study of cases in the wards, although it [the system as it had developed] may need to be modified in certain particulars it cannot be dropped; it has come to stay."

That is the position which, *pace* "Academicus," I submit, Sir, in view of the paramount necessity for training the student, must be accepted, and, indeed, has been commonly accepted, in the selection of occupants for clinical chairs in London University, and I see no prospect of change in it unless and until quite other conceptions prevail of the proper purpose of medical education. So long as the medical schools are required to administer a curriculum which becomes heavier with every successive revision, and has long outgrown any possibility of assimilation in the time allotted to it, teachers will be forced to concentrate on routine instruction of students (hackwork, "Academicus" unkindly calls it), and medical research, if done at all, will be done elsewhere than in the teaching hospitals, which must inevitably regard as their first and inescapable obligation the training of the medical student for general practice.—I am, etc.,

House of Commons, April 5. E. GRAHAM-LITTLE.

Acute Dilatation of the Stomach

SIR,—As one of the smaller fry, I was heartened and encouraged by Mr. Harold Dodd's letter (*Journal*, April 1, p. 695) emphasizing the occasional difficulty of passing a duodenal tube. Obviously, if the patient either can't or won't swallow it one can't make him. There is all the difference in the world between getting a "test-meal" patient tranquil and free from abdominal distension to swallow the tube, and struggling to persuade a sick and terrified child, perhaps after a laparotomy for generaiized peritonitis, to swallow yards of slimy rubber tube while being violently sick. But it is just in this sort of case that the duodenal tube is life-saving, and I am extremely grateful for Mr. Dodd's suggestion of decicaine lozenges.

After a bitter personal experience I made a solemn vow never to try to pass a duodenal tube on a sick patient unless the pharynx had been sprayed with cocaine, but even this has failed on occasion, and one has had to adopt the very dangerous expedient of anaesthetizing the patient with chloroform, and at the critical moment rapidly pushing down a large-bore tube.—I am, etc.,

Oakham, Rutland, April 4. GORDON PURDY.

Medical Women's Questionary

SIR,—May we ask the courtesy of your correspondence columns to call the attention of medical women to the questionary on the kinds of work being undertaken by them which has recently been circulated by the Medical Women's Federation to all women on the *Medical Register*.

A similar questionary on a much smaller scale was sent out ten years ago, but this is now quite out of date and useless for reference. Inquiries are frequently received regarding the number of women employed in various branches of medical work, the number married or retired from practice, etc., and to obtain any kind of accurate knowledge it is necessary that replies should be received from as large a proportion of medical women as possible.

May we appeal to any who have not yet completed the form to do so at once and to send it to the Medical Women's Federation, 9, Clifford Street, Bond Street, London, W.1. Extra copies of the questionary, if required, will be sent on application.—I am, etc.,

London, W.1, April 5. VIOLET KELYNACK.

Shock Therapy

SIR,—May I be allowed to write one more letter in reply to Dr. Ian Skottowe (April 1, p. 697). He appears to believe that I think that histamine is the only active agent in the treatment. That is not according to fact, for insulin is used in combination with the histamine, and this makes all the difference. Moreover, it is of no importance at all what I think myself, but it is the opinion of others who use my method that matters. Full details of the method and treatment have been sent (only by request, of course) to such responsible bodies as the Government medical departments and hospitals, State hospitals, medical schools, and clinics in Australia, South Africa, India, America, Ireland, and a number of centres in England. The results which I have received from some of these places are so far satisfactory and coincide with mine. Apart from being free from danger, the method appears to give satisfaction also in other ways.—I am, etc.,

HORACE HILL,

Salisbury, April 2. Medical Superintendent, Laverstock House.

Medical Notes in Parliament

The House of Commons adjourned on April 6 until April 18, and the House of Lords stood similarly adjourned, but both Houses, because of events in Europe, were recalled for April 13.

During the debate on the adjournment in the Commons on April 6 the British policy toward refugees was discussed. Mr. HAROLD NICHOLSON said that those who worked for the refugees found little help from anyone in this country in placing the finest scientific research students in the world. Sometimes they could have them *en bloc*—the whole staff of a laboratory. As for ordinary doctors he held it madness not to avail ourselves of qualified medical men, in view of the conditions in the British Colonies or in Egypt. Mr. MANDER suggested that refugee doctors should be allowed to assist in the British scheme of national service. Lord WINTERTON, in reply, said that it was hoped that discussions on the migration of refugees would soon be resumed between the German Government and Mr. Pell, the American Vice-Director of the Evian Committee on Refugees.

CIVIL DEFENCE BILL

Provision for Treatment of Casualties

The second reading of the Civil Defence Bill was moved by Sir JOHN ANDERSON on April 4. Mr. WEDDERBURN, speaking on the application of the Bill to Scotland, said the Scottish Office had surveyed all the hospital resources of the country, whether controlled by local authorities or voluntary bodies, and also hotels and schools which might be used for temporary hospitals. Altogether 500 hospitals, institutions, and other buildings had been inspected, and the hospital officers of the Department of Health in Scotland had since

been in contact with medical officers of health and superintendents of hospitals about the problems of bringing particular institutions into the emergency scheme. Plans had been drawn up for the provision of beds for wounded people in existing hospitals and for adapting other buildings to make them of use for the treatment of casualties. When the delivery of medical and surgical equipment which the Department was providing had been completed they would be able to find 12,000 beds for wounded persons in the first twenty-four hours of war and another 8,000 by the end of the first week. They were planning another 6,000 beds in hutted units to be erected on lands adjoining existing hospitals and institutions, and were immediately beginning negotiations with the hospital authorities concerned. The Scottish Central Emergency Committee for the nursing profession was appointed in January to register nursing personnel in Scotland and to arrange for their training. Local committees were being set up to arrange for training of nursing auxiliaries, and a register of trained or semi-trained nurses was being compiled by the College of Nursing. A number of country houses had been offered for use as hospitals in the event of war, and had all been inspected by hospital officers of the Department. Orders had been placed for bedsteads, mattresses, and blankets, and delivery of these had begun or was about to begin. The full requirements of every hospital which came into the scheme had been ascertained in the way of medical or surgical or ward equipment, and all the hospitals had been asked to maintain reserves of stores. The Department had also ordered drugs, dressings, and surgical equipment, the delivery of which would begin that week. The Scottish Motor Traction Company had arranged for the conversion of seventy-five buses into ambulances, each to carry eight stretcher cases, and the necessary fittings had been manufactured and were held in readiness. The Department had 2,500 stretchers available for this service, and most were stored in suitable places. It hoped soon to publish a confidential memorandum describing the emergency hospital services in Scotland.

TRANSPORT QUESTION

Colonel NATHAN asked how far arrangements had gone for the treatment of casualties, and whether 300,000 beds would be available in England and Wales if an emergency arose suddenly. That figure had been mentioned by Mr. Bernays in a speech at Bristol, but was the Minister sure that the beds were there with blankets, sheets, pillows, and all the necessary furnishing? Was it not a fact that a number of places scheduled for use as hospitals were at present public assistance institutions, and when the emergency came would either not be available or not suitable for use as hospitals? In some instances whole floors had been scheduled for use as hospitals where there was no water and perhaps no light. The Ministry of Health proposed a figure of £1,500,000 for structural precautions and adaptation in hospitals. He had been told that the figure involved for carrying out this scheme and the directions given by the Minister in Emergency Regulations Pamphlet No. 1 was not £1,500,000 but £50,000,000. The number of nurses at present available was estimated at 80,000 to 100,000, but if the dispersal of the nursing staff was wide those numbers must be largely increased. Evacuation of hospital casualties rested on getting the injured who would only be patched up away to the country for proper treatment in order to make room for the next batch; were they to go by train or by road? It would be difficult for hospitals to get their patients to the river. A group of hospitals for which he had a certain responsibility would have to evacuate all its patients by Hammersmith Bridge or by Putney Bridge, and he wished to know what steps were being taken to provide one-way traffic and to arrange for temporary bridges or pontoons if the existing bridges were bombed.

Sir FRANCIS FREMANTLE spoke on the necessity for issuing identification disks bearing the name and reference number of the wearer. The Bill should give local authorities permission to issue these disks. The total number of nurses now nursing in London would start only one of the thirteen sectors laid down for the defence of London. Not nearly enough women were being trained.

Dr. BURGIN said arrangements had been made for ambulance trains by the conversion of parcel vans and for road transport by Green Line coaches which had already been adapted.

EVACUATION PLANS

The debate was resumed on April 5. Dr. ELLIOT stated that there were enough gas masks for the whole population, but some of those issued in September had been damaged, and therefore a reserve was being built up for use in London. The first necessity was to keep a reserve in the dangerous areas. The billeting survey had revealed some 5,000,000 places, and the Government was arranging for the movement of 3,000,000 persons without interfering with a further 1,000,000 who, under private arrangements, had reserved accommodation in the reception areas. The 3,000,000 included 2,500,000 children. They would be moving 1,000,000 people a day. He wished to concentrate on the smooth and easy movement of these 4,000,000 people, and thereafter to consider further evacuation in the light of conditions when the war began and when railways and roads might be broken up. Nothing would be worse than a rigid plan which broke down in its early stage.

Replying to the inquiry made by Colonel Nathan about the provision of beds, he said the figure was 200,000 beds for immediate use, and another 100,000 in a few days after an emergency developed. There was no doubt they could get 200,000 beds at once which, most important of all, would have nurses. The extra 100,000 were on order, and 25,000 had been received and distributed to hospitals. There was a need for nurses. Eighty thousand were available, but three times that number might be required before the war was over.

HUTMENTS AS AUXILIARY HOSPITALS

The Ministry had started on the programme of auxiliary hospital accommodation in the form of hutments. It had made arrangements for 20,000 beds to be proceeded with forthwith. The three main areas for these would be London, Birmingham, and Tyneside, and he hoped to co-operate with the local authorities and the voluntary hospitals so that full use might be made in peace-time of this reinforcement to the hospital accommodation of the country. He hoped to co-operate with those at present appealing on Tyneside for extra hospital accommodation.

The Bill was then read a second time without a division. The House then went into committee on the money resolution in connexion with the Bill. This resolution included authorization of a grant to county and county borough councils, etc., towards approved expenses in rendering premises suitable for the hospital treatment of casualties or for the purpose of protecting persons in hospitals from injury in the event of hostile attack. Sir ARNOLD WILSON remarked upon a passage in the resolution authorizing a payment towards "provision of a bacteriological service for controlling the spread of infectious disease in the event of war." He said that to prevent the spread of infection in time of war a whole organization would be needed which would not be included within the limits of a bacteriological service. Replying to the debate Sir JOHN ANDERSON said the Ministry of Health was undertaking the detailed organization of casualty services in hospitals and of a bacteriological service, and this financial provision was required only to enable him to carry out his duty in that respect.

After further discussion of the finance of the measure the House agreed to the money resolution.

Safety in Mines

Safety in mines was discussed by the House of Commons on March 21, on the report stage of the Civil Estimates (Vote on Account). Mr. DAVID GRENFELL, opening the debate, pointed out that although 450,000 men had been withdrawn from the industry in the past twenty years the output of coal stood higher; this was due to the increased use of machinery. An increase was shown in the death and disablement rates from accidents over this period. Apart from loss of life from

explosions, other accidents had shown a steady tendency to increase. Mr. ERNEST EVANS said that whereas the mortality rate among miners compared favourably with the general mortality rate among the male population some time ago, it was now relatively worse, and the general level of casualties among miners had been static during the last fifteen years.

Mr. CROSS, replying for the Government in the absence through illness of Captain Crookshank, said the statistics for 1938 showed little or no improvement regarding deaths or serious injuries in mines as compared with 1937. As a result of the Royal Commission's report proposals for strengthening substantially the Health and Safety Provisions of the Mines Department and the Headquarters of the Inspectorate were under discussion. In the meantime consideration was being given to matters which could be dealt with administratively, but little progress could be made without additional staff. Advance action might be taken in measures to improve the ventilation in safety-lamp mines and to require the air current in such mines to be sampled systematically, and the enforcement of suitable precautions against the inhalation of dangerous dust.

Sir ARNOLD WILSON said the general mortality rate of miners would not compare unfavourably with that of the population of England and Wales but for the unsatisfactory conditions, external altogether to the pit, in which the miners lived. It seemed clear that there had been a deterioration in the general expectation of life of miners in the last thirty years, apparently through factors external altogether to the mines. On the question of temperature, Dr. Vernon had shown that the accident rates varied directly with the temperature if that was over 70°, wet bulb. It was not expensive to provide cool air and dry air if the wet bulb temperature went above 70°. He was sure mine owners could find means of providing this if they were put to it. He submitted that the Royal Commission might have made more of the evidence presented to them on silicosis. His impression was that parts of the increase in silicosis were due to increased liability to the disease of men who entered the industry though not physically fit. It was a scandal that workmen who complained of silicosis had to pay for a medical examination if the result was not positive. It ought to be possible for any man to have a free medical examination, and that step would reduce the incidence of industrial disease in mines.

After further discussion a motion to reduce the estimate was withdrawn and the report stage concluded.

Tuberculosis Incidence among Seamen

Mr. OLIVER STANLEY told Mr. James Griffiths on March 28 that he had seen the paragraph on the high incidence of tuberculosis among seamen in the report of the Committee of Inquiry into the Anti-tuberculosis Service in Wales, and the view expressed therein that this high rate was due to the unsatisfactory accommodation of seamen on ship and on shore. It took into account other factors in addition to accommodation on board ship, and was based on statistics for the years 1924 to 1935. All aspects of crew accommodation were reviewed in 1937 by the Board of Trade in consultation with the other interests concerned, and as a result new crew space instructions were issued by the Board. The condition of the accommodation for seamen on shore was a matter for the local authority concerned.

Superannuation of Local Government Officers

Mr. BERNAYS, in the House of Commons on March 29, moved the second reading of the Local Government Superannuation Bill. He said its purpose was to amend provisions of the Local Government Superannuation Act, 1937, and the Local Government Superannuation (Scotland) Act, 1937. The former came into operation on April 1, 1939, and the latter would come into operation on May 16, 1939. Since their passage it had become apparent that in three minor points the language of the Acts did not carry out the intention of Parliament, and the object of the Bill was to remedy these mistakes. Clause 1 dealt with the question of reciprocity between England and Scotland by redefining service as "service rendered to any local authority in either country." Clause 2 referred to England only. The superannuation of persons employed in certified institutions for mental defectives was in general provided for under the Asylums Officers Superannuation Act, 1909, as extended by the Asylums and Certified Institutions (Officers) Pensions Act, 1918. These employees were excluded from the Act of 1937 with the exception of certain Poor Law employees, who remained subject to the Poor Law Officers Superannuation Act, 1896. A certain number of Poor Law employees had exercised an option to transfer to the Act of 1922, and since that Act was repealed from April 1 they would cease to have any superannuation rights. The object of Clause 2 was to bring this class of employees within the scope of the Act of 1937 and thus preserve their rights.

On March 31 the Bill passed through committee without amendment and was read a third time without further discussion.

Sanitary Control of Proposed Camps

Dr. ELLIOT, in the House of Commons on March 29, moved the second reading of the Camps Bill. He said the financial provision for the Bill of £1,200,000 was considered sufficient for fifty camps each to accommodate about 350 children at a time. It was intended that seven camps would be in Scotland and the remainder in England and Wales, and they would be built as a supplement to accommodation for evacuation from vulnerable areas. No number of camps that could be constructed in the near future would prove an alternative to billeting, as the school children alone from the evacuation areas numbered about 1,500,000, not to mention younger children and their parents. At present twenty permanent camps for school children in England and Wales were provided by local education authorities and contained accommodation for roughly 1,400 children. About half these camps were used for undernourished and weakly children drawn from poorer homes, and the remaining camps for groups of normal children from town schools. The present proposal would be an enormous advance, and as the camps would be heated it was hoped that they would be used for a large part of the year.

Dr. HADEN GUEST considered the building of camps was urgent, and the House should concentrate more on their use for the evacuation of school children. When a camp was provided for 350 children water supply, sanitary accommodation, and cooking accommodation could be so planned that with little adaptation the camps could be used for ten times that number.

Sir FRANCIS FREMANTLE objected to the provision in Clause 3 of the Bill which exempted areas acquired or appropriated for the purpose of these camps from restrictions imposed under the Town Planning Acts or the Town and Country Planning Act. He said these camps would involve a danger to health, especially as regards conveyance of infection, unless properly supervised from the health point of view. At any time they might get an outbreak at one of these camps—such an outbreak as occurred at the Crystal Palace at the end of the last war—which might carry off perhaps half the children. The rural medical officer of health, with other duties to perform and more likely not a whole-time officer, would probably not be used to dealing with these matters on a big scale. The county medical officer ought to be in command, but under the existing law he had no direct responsibility for sanitation. A provision was needed whereby the county medical officer of health or the county council through its sanitary committee should be responsible for the sanitary control of the camps.

Mr. WEDDERBURN dealt with the application of the Bill to Scotland, and it was then read a second time without a division.

Scottish Board of Control

The Reorganization of Offices (Scotland) Bill was considered on report by the House of Commons on March 30. On Clause 2 (Reorganization of the General Board of Control)

Mr. COLVILLE moved to increase the number of members of the Joint Board of Control from six to seven in addition to the chairman. He proposed to enlarge the Board so as to include one other lay member. The clause provided for at least two medical commissioners, and it was not intended, by this amendment at all events, to appoint more than two. If the amendment was adopted the Board would consist of the chairman, who would be an assistant secretary to the Department of Health, two medical commissioners, one legal commissioner, who would probably be part-time, one representative of the Scottish Education Department, and three laymen. The House agreed to the amendment.

The report stage concluded, and Mr. Colville moved that the Bill be read a third time. He pointed out that in Clause 2, Subsection (8), opportunity had been taken to provide that the method of appointment of the clerks of the Board of Control by the Board with the approval of the Secretary for Scotland should also extend to the secretary of the Board of Control. At present the secretary's appointment was a Crown one, but the King had consented to the change, and the appointment would lie with the Board of Control subject to the approval of the Secretary of State.

The Bill was then read a third time.

Medical Advisory Committee on Cancer

Sir FRANCIS FREMANTLE asked the Minister of Health to state the names of the members of his medical advisory committee and also the names of the members of the subcommittee to advise him on general principles in regard to the treatment of cancer and to consider any questions referred to them by him.

Dr. ELLIOT replied on March 31 that the Medical Advisory Committee, which was under the chairmanship of Sir Arthur MacNalty, Chief Medical Officer of the Ministry, was constituted as follows:

Sir Comyns Berkeley, M.D., F.R.C.P., F.R.C.S., F.R.C.O.G.
Sir Henry Brackenbury, M.D.
Viscount Dawson of Penn, M.D., F.R.C.P.
Lord Horder, M.D., F.R.C.P.
Dr. Robert Hutchison, P.R.C.P.
Sir Kaye Le Fleming, M.D.
Mr. Hugh Lett, P.R.C.S.
Sir Ewen Maclean, M.D., F.R.C.P., F.R.C.O.G.
Sir Edward Mellanby, M.D., F.R.C.P., F.R.S.
Sir Frederick Menzies, M.D., F.R.C.P.
Professor W. Fletcher Shaw, M.D., P.R.C.O.G.

Invitations to serve on the Cancer Subcommittee of this committee, which was to advise the Minister on general principles in regard to the treatment of cancer and to consider any questions referred by him to it, had been accepted by:

Sir Farquhar Buzzard, M.D., F.R.C.P.
Dr. R. Veitch Clark, D.P.H.
Dr. Ivor J. Davies, F.R.C.P.
Mr. M. Donaldson, F.R.C.S., F.R.C.O.G.
Dr. Mary Gilmour.
Sir Charles Gordon-Watson, F.R.C.S.
Mr. D. G. Greenfield, M.D., F.R.C.S.
Dr. R. Hilton, F.R.C.P.
Professor R. E. Kelly, M.D., F.R.C.S.
Professor E. L. Kennaway, M.D., F.R.C.P., F.R.S.
Sir Frederick Menzies, M.D., F.R.C.P.
Mr. J. Ralston Paterson, M.D., F.R.C.S.Ed.
Professor J. A. Ryle, M.D., F.R.C.P.
Professor W. Fletcher Shaw, M.D., P.R.C.O.G.
Mr. H. S. Souttar, D.M., F.R.C.S.
Mr. G. F. Stebbing, F.R.C.S.
Professor M. Stewart, M.B., F.R.C.P.
Sir Cuthbert Wallace, F.R.C.S.
Dr. Constance Wood.

Dr. Elliot added that Sir Arthur MacNalty would also act as chairman of the Cancer Subcommittee.

Dietary of School Children

Mr. KENNETH LINDSAY told Sir William Jenkins on April 3 that the dietitian appointed by the Board of Education had visited feeding centres and schools since February, 1938. An important part of her function was to suggest improvements in the dietary where the meals provided were unsatisfactory, but no fixed daily or weekly diet was recommended as the necessary constituents could be provided in a variety of ways. The provision of dinners, whether free or on payment, was primarily a matter for the local education authorities, but representations were made by the Board to the authorities where they considered the provision of dinners desirable. In such cases the dinners were usually given in addition to, and not in substitution for, the supply of milk. The recognition that malnutrition was one of the causes of mental retardation was the basis of the statutory provision of meals for children who "are unable by reason of lack of food to take full advantage of the education provided for them." No statistics were available of the number of children who were affected by mental retardation, due to malnutrition, in elementary or secondary schools. There were many other causes of mental retardation, some associated with and some distinct from malnutrition. Reliable statistics on this exceedingly difficult matter were unlikely to be secured.

Asked what number of Scottish school children received free meals and free milk, respectively, in 1938, as compared with 1933, Mr. COLVILLE circulated the following table and notes:

Year	No. of Children who Received		Net Cost to Education Authorities	
	Free Meals	Free Milk	Free Meals	Free Milk
			£	£
1933 ..	26,220	—	19,458	—
1938 ..	33,763	64,434	30,446	22,024

Notes:

(1) In 1933 there were no special schemes for the provision of milk in operation; where milk was provided, it was classified under the heading of meals.

(2) The number of children who received free meals or milk includes children who were provided for by voluntary agencies.

Opticians in the R.A.M.C.

Mr. RHYS DAVIES asked on April 3 what were the qualifications of persons described as registered opticians, Royal Army Medical Corps, and what steps were taken to see that the public was not misled by the incorrect use of such a description. Mr. HORE-BELISHA replied that a number of personnel were specially enlisted as opticians during the war on producing evidence of having passed either of the following examinations: (*a*) Associate Member of the British Optical Association; (*b*) Fellow of the Worshipful Company of Spectacle Makers. He had no power of preventing the incorrect use of the description quoted by Mr. Rhys Davies.

Transfer of Broadmoor to Board of Control

On April 4 the remaining clauses of the Criminal Justice Bill were considered and approved by the standing committee of the House of Commons to which the Bill had been referred. On Clause 68 (Transfer of control and management of State Mental Hospitals to the Board of Control) Sir ARCHIBALD SOUTHBY opposed the transfer of Broadmoor to the Board of Control. He said that at present Broadmoor Criminal Lunatic Asylum was administered under the Home Office. It was regularly inspected by the Board of Control and their reports had been uniformly satisfactory. A medical specialist governed the institution, and a Council of Supervision, consisting of leading residents in the district, visited the asylum and dealt with all questions of staff, discipline, etc. For more than sixty years that council had discharged its duties to the satisfaction of the Home Office. The Bill now proposed to transfer control from the Home Office to the Board of Control except in respect of discharge of any patients considered to be cured, which remained a responsibility of the Home Office. Under this Bill it was also desired to abolish the Council of Supervision. He contended that means should be found to retain the council under the Board of Control or under the Home Secretary.

Sir SAMUEL HOARE said this Council of Supervision had done valuable work, but a change had to be made as a result of the repeal of the Statute of 1860, which had given that council powers that, if interpreted in extreme form, would confer on it the actual management and treatment of mental patients. In practice those powers had never been exercised. A technical change was necessary, but he undertook that after

the transfer to the Board of Control the interest of these local residents would be retained, and he hoped they would continue to do much the same kind of work as now but as a committee of visitors, while leaving the technical and medical side of the institution to the Board of Control.

Sir ARCHIBALD SOUTHBY accepted the assurance and Clause 68 was agreed to. On Clause 70 (Removal of State mental patients for various purposes) Mr. RHYS DAVIES drew attention to a subsection which authorized the removal of a State mental patient to a hospital or other suitable place for the purpose of medical or surgical treatment. He wished to know whether the Home Office would have a right to order an operation without the permission of the relatives. Sir SAMUEL HOARE said that treatment and operations contemplated under this clause would never be undertaken without the consent of the relatives. That was the present state not of the law but of the practice, and he was told that no surgeon would undertake an operation without the consent of the relatives.

The clause as amended was agreed to.

On Clause 78 (Interpretation) the committee accepted an amendment, moved by Sir TERENCE O'CONNOR, that "'mental treatment' includes in relation to any person any treatment designed to cure or alleviate his mental condition." The committee also accepted an amendment that "'mental hospital' includes a State mental hospital," and the clause as amended was approved. On the clause dealing with application to Scotland (79) an amendment was accepted that in this application "for any reference to a mental hospital other than a State mental hospital there shall be substituted a reference to an asylum." Other small amendments were made and the clause added to the Bill. The committee then adjourned until April 20.

Insurance Medical Certificates in Compensation Cases.—Dr. ELLIOT said on March 23 that health insurance medical certificates, which were the property of the insured persons to whom they were given, were in some cases shown to employers either in connexion with a claim for workmen's compensation or for some other purpose. He did not think there could be any objection to an arrangement of this nature, provided that it did not interfere with the prompt dispatch of the certificate to the man's approved society.

Water Supply in Evacuation Areas.—Replying on March 23 to Mr. Sorensen, Dr. ELLIOT said that in the evacuation arrangements steps would be taken to avoid areas where insuperable difficulties were likely to arise on account of water supply or drainage. It was not considered that the number of such areas would be large. He was not satisfied that there was any case for grant from national funds for the purpose of improving water supply or drainage in rural areas as part of the evacuation scheme.

Slate Quarrying and Silicosis.—Replying to Mr. James Griffiths on March 28, Mr. R. V. GRIMSTON said an expert medical inquiry had revealed no evidence of silicosis among slate workers in open quarries. About a year ago the attention of Capt. Crookshank was called by the chairman of the Committee on the Anti-tuberculosis Services in Wales to statements which had been made about conditions in the slate mines. Special investigations were at once undertaken. As a result considerable improvements in the methods of dust suppression had been effected. The matter continued to have the close attention of inspectors of mines.

Deaths following Measles Serum Inoculation.—On March 30 Mr. LEACH asked Dr. Elliot where the thirty-seven cases, with seven deaths, after inoculation with convalescent measles serum, referred to on page 39 of the last annual report of his chief medical officer, occurred, whether any inquests were held on the fatal cases, and whether his medical officers held an inquiry into the circumstances. Mr. BERNAYS, answering for the Minister, said eighteen of these cases occurred in Oxfordshire, eleven in Essex, six in Berkshire, and one each in Surrey and Sussex. An inquest was not held on any of the fatal cases. Investigations into this group of cases, which were of a highly exceptional character, were still being carried out by the medical staff of the Ministry of Health with the assistance of representatives of the Medical Research Council and other medical authorities. He could not at present make any statement about the publication of a report.

Gas Protection for Babies.—Answering Mr. Poole on April 4, Sir JOHN ANDERSON said the gas-bag for very young children was intended for babies under 2 years of age. Contracts for the manufacture of the various components had been placed, but no issues had been made. He could not say when the issue would be completed.

Deaths following Vaccination or Vaccinia.—Eight death certificates were received in 1938 on which death was attributed wholly or in part to vaccination or vaccinia; the ages of the dead persons being respectively 1 month, 14 years, 16 years, 1 month, 5 months, 5 months, 4 weeks, and 4 months. In the first four of these the death was classified as vaccinia.

Welsh Board of Health.—Dr. Elliot states that, as recommended by the Committee on Anti-Tuberculosis Services in Wales, the position of the Welsh Board of Health will be reviewed.

There were three deaths from small-pox in England and Wales during the year 1938, two of males aged 20 and 73 respectively, and one of a female aged 23.

Obituary

T. E. KNOWLES STANSFIELD, C.B.E., M.B.

Dr. Thomas Edward Knowles Stansfield, who died in Paris on February 19 aged 76, had been a member of the British Medical Association for forty years, and was president of the Kent Branch in 1921–2. Sir Hubert Bond sends the following appreciation:

Although it is some eighteen years since Knowles Stansfield retired from the practice of his profession, and for more than half of that time he had lived abroad, the force of his enthusiasm and work is by no means spent. Not a few of the present-day arrangements for treatment in mental hospitals in this country directly or indirectly owe their origin to his administrative genius: some were devised by him, and others, which had either been suggested to him or he had observed abroad and whose importance he had been quick to see, owe their adoption, secured sometimes in the face of opposition and misunderstanding, to the forcefulness of his drive and to a strength and tenacity of purpose and decision which to the end were characteristic of him.

Knowles Stansfield came of a very old family long settled on one or other side of the contiguous borders of Lancashire and Yorkshire, and whose dwellings ranged from village cottages to their ancient seat, Stansfield Hall, Todmorden. His decision to read medicine was taken later than is usual. Indeed, it is not certain what studies he pursued during the one year he spent at Owens College, Manchester, before he went up to Edinburgh, where he graduated M.B. in 1889. He had a bent towards pathology, and for some months worked in the pathological department of the Royal Infirmary. It was just about this time that much-increased attention was being given to the pathology of mental disorders, the hub and home of which was the West Riding Asylum at Wakefield. In 1889 the London County Council came into being, and as part of its newly established asylums department had inherited a scheme for the erection of a new mental hospital

(Claybury); within the latter's curtilage, and backed by the powerful advocacy of Sir William Collins, there was established in 1895 the central laboratory for the London mental hospitals with the late Sir Frederick Mott as its director. Among the other institutions taken over by the Council was Banstead Asylum, opened in 1877 and designed for 1,700 so-called chronic cases of insanity; in more than one way it had realized and demonstrated the evils of an institution designed to admit only patients transferred from other mental hospitals—a limitation that subsequently was rectified. In the year 1890 there was created at Banstead the post of fourth assistant medical officer and pathologist—really that of junior assistant with an additional salary for the pathological work, which was mainly that of responsibility for post-mortem examinations—and hitherto, instead of "pathologist" this junior medical position had had as its suffix "and dispenser." If the title "pathologist," as used here, did not connote in fact much, it at least was a sign of the times. It was a part of the advertisement which specially attracted Stansfield's notice, and he became the first occupant of this new post at Banstead. Without any previous thought of taking up mental hospital work, his interest in it was quickly roused; indeed, it became absorbing, especially upon its administrative side. It was far from limited to that aspect, however; and two items of his work deserve mention. Fresh from Edinburgh, where he had been accustomed to see routine appeal to the aid of the laboratory, either in clinical diagnosis or in extension of post-mortem work, and where as a clinical clerk in the wards he had been subjected to a rigid discipline in the mode of note-taking at the bedside, it struck him as anomalous that the big institution in which he now found himself should be without a laboratory; and, again, that clinical notes should be entered in case-books kept in offices far distant from the wards and made from material which had been committed temporarily to memory or jotted down on scraps of paper or, at best, in a rough pocket notebook. He was fortunate that at the head of the hospital was one of the most distinguished superintendents of the day, who also both lectured at Bart's and examined at the University of London for the M.D. in psychological medicine—the late Dr. T. Claye Shaw. The latter, only too ready to give rein and scope to enthusiasm, was able to set aside a small room and to get it equipped as the laboratory. He approved, too, of Stansfield's desire to have provided in every ward a stoutly bound notebook. In these substantial books the doctors could write their notes, either at the bedside or *vis-à-vis* the patient in perhaps a corner of the day-room (there were no clinical rooms); and later on in the day the notebooks were conveyed to one of the offices to be copied, by the dispenser, into the statutory case-books. This innovation, simple enough in itself, was the forerunner of the loose-leaf system of clinical records as instituted several years later at Bexley, which, with or without modification, has spread to almost every mental hospital in the country and which, far from being just a piece of clerical administration, embodies important medical and other scientific principles.

Claybury was opened in 1892–3 as the fifth asylum for the newly constituted administrative county of London. It was the first mental hospital in this country to be designed and opened for 2,000 patients. To preside over this hospital someone of distinction and of proved administrative capacity was needed, and the then superintendent of Earlswood Asylum, now Sir Robert Armstrong-Jones, was appointed. In 1895 Stansfield was transferred to Claybury as first assistant medical officer. At the age of 31 and with no more than three years' mental hospital experience, to find himself in such a position of vantage was indeed a stroke of fortune. Not, however, that Stansfield was any mere favourite of fortune; far from it. For, his interest swiftly caught, his application and output of energy were prodigious, displayed with an obvious determination to forge ahead and to fit himself for speedy promotion. It is to be doubted if any other example can be found of a mental hospital physician of his juniority so systematically devoting so large a share of annual leave to professional self-improvement by the visiting of other mental hospitals at home and abroad. His reports on mental hospitals and clinics in America and Germany were printed and circulated, and in 1896 he read a paper, "Hospital Treatment of the Insane," before the Psychological Section of the British Medical Association at its meeting at Carlisle. There was no pigeon-holing of these reports: there were at the time ears upon which their message fell with ready belief and response, and the opportunities he had of orally reinforcing the views in them not only enhanced his position but contributed largely to his appointment in 1898 as the first superintendent of Bexley Mental Hospital (Kent). The adoption of the more important of the views they contained has played a definite and notable part in the lines on which public mental hospitals in England and Wales have developed.

Dr. Stansfield was twenty-three years superintendent at Bexley. Foremost among the changes he urged were the adoption of the cottage (or villa) system in the design of mental hospitals, and the siting of such detached buildings so that recent cases, themselves adequately subdivided, would be well separated from others. One of his more salient activities was the further development of the villa system, including the use of parole and the separation of recent from other cases. Classification of patients throughout the whole hospital was on carefully considered lines and strictly maintained. Great use was made of the open-air treatment in bed on glass-roofed verandas. The development of utility occupations for the patients at Bexley gradually formed a leading part in the hospital's administration.

In the centennial year of the announcement by Schleiden and Schwann of the cell theory, death claimed on March 3 a man whose treatise *The Cell in Development and Inheritance* (1896) profoundly influenced biological thought in the science of cytology. Dr. EDMUND BEECHER WILSON, for thirty-seven years Da Costa Professor of Zoology at Columbia University in New York City, was born in 1856, and after graduating from the Sheffield Scientific School of Yale University in 1878, pursued post-graduate studies at Johns Hopkins, Cambridge, Leipzig, and Naples. A polished lecturer and a writer of good English, he enriched science with valuable observations in the fields of experimental embryology and of heredity. Innumerable honours came his way. He was President of the American Association for the Advancement of Science in 1913, and in the following year delivered the Croonian Lecture in London. It was said of him that he was an honorary member of learned societies in nearly every civilized country.

The passing of Dr. JOHN MCCLINTOCK, who died at his home in Church Stretton on March 12, has robbed Shropshire of yet another of those outstanding personalities who contributed so largely to the noblest traditions of the profession in the county. Born in 1875, the son of John Robert McClintock of Perthshire, he studied medicine at King's College Hospital and at Edinburgh, where he qualified in 1899. In 1900 he settled in Church Stretton, and in the

same year became a member of the British Medical Association, serving later for many years on the Council of the Shropshire and Mid-Wales Branch. In addition to a very extensive and widely spread practice he was resident medical superintendent at the Grove House Private Mental Hospital. To know Jack McClintock (writes J. A. A.) was to love him, for there was no touch of unkindness in his whole nature. In his work he never spared himself, but gave his unstinted devotion to his patients, to all of whom he was not only physician but counsellor and friend. His loyalty and essential goodness, combined with a geniality and an unfailing humour, gained him the respect and affection of patients and friends alike. To many hundreds of these he will ever be remembered as a beloved physician, a great sportsman, and a gallant gentleman.

Many readers will learn with regret of the death of WILLIAM EDWARD SARGANT, which took place on March 29 at Amersham, Bucks, where he was living in retirement after his long period of service at St. Bartholomew's. The eldest son of William Sargant, he was born in 1863, and before entering as a medical student at Bart's with a preliminary science exhibition he had been in employment in the City, thus joining at a later age than his contemporaries. He qualified M.R.C.S., L.R.C.P. in 1893, and was for some time senior clinical assistant in the old orthopaedic department. Then for many years he combined the duties of Manager of *St. Bartholomew's Hospital Journal* and those of School Registrar, in the course of which he arranged all the appointments of dressers in the surgical wards. He was most conscientious in the performance of all his tasks, and students and staff alike found him pleasant and easy to work with. In time he built up a private medical agency, mainly for the benefit of old Bart's men, and many men now in successful practice owe their early appointments as assistants, locumtenents, or partners to his introduction. He retired from the College Registrarship two or three years before Dr. T. W. Shore left the Deanship in 1930. An unpaid post to which Mr. Sargant gave much time and trouble was that of honorary secretary for cases in the work of the Royal Medical Benevolent Fund during the treasurership of Dr. Samuel West.

We regret to record the death of Dr. ALFRED BROWN of New Barnet, who had been a member of the British Medical Association for fifty-three years. A student of the University of Aberdeen, he graduated M.A. in 1880, took the M.B. and C.M. degrees in 1883, and proceeded to the M.D. in 1892. Dr. Alfred Brown practised for some time in Manchester and held the appointment of consulting physician to the Greengate Children's Hospital and the open-air hospital at Salford. He was a member of the Manchester Literary and Philosophical Society and a past-president of the Manchester Clinical and Pathological Societies. He contributed to the *British Medical Journal* in 1894 a paper on oedema of the larynx occurring during an attack of rheumatism, and he was a member of the Laryngological Section of the Royal Society of Medicine; he also published a paper on the aetiology and treatment of lobar pneumonia in the *Transactions of the Manchester Clinical Society*. The funeral took place at the Parish Church, Bowden, Cheshire, on March 8.

Dr. GERHARD WAGNER, the head of the German medical services, has died in Munich at the age of 50. He was the leader of a small group of German doctors who ten years ago formed themselves into the National-Socialist Medical League, and he became Herr Hitler's confidential adviser on national health matters, and was afterwards nominated Reich medical leader.

Dr. ANGENETTE PARRY, past-president of the American Medical Women's Association, for many years a New York obstetrician and a close friend of Mark Twain, died of a cerebral haemorrhage on March 1 in the Massachusetts General Hospital, Boston, aged 81.

Medico-Legal

DERMATITIS FROM CLOTHING

Two interesting actions arising out of dermatitis caused by clothing have been considered recently by the High Court. It is always somewhat difficult to prove the connexion of the disease with the article against which the offence is alleged.

In Mayne *v.* Silvermere Cleaners, Ltd., heard before Mr. Justice Croom-Johnson at Liverpool Assizes, a man wore a blue suit without harm for two and a half years and sent it to the cleaners. Owing to a death in his family he put the suit away when it was returned in the tissue-paper wrapping applied by the cleaners. After six months he wore it for a few days, and then noticed redness and irritation of his wrists, and afterwards on the back of his neck and behind his knees, although he had given up wearing the suit. A doctor gave evidence that he had applied to the patient's skin, as a test, patches from the coat, its lining, and another similar cloth; pimples appeared at the site where the cloth of his own suit had been applied and not at the other sites. An analyst said that he found in the suit the irritant substance quinondichlordiamine. On the other hand Professor W. H. Roberts, president of the Society of Public Analysts, said that he could find no evidence of any irritant substance, and certainly none of the substance which the patient's witness said he had found. The judge, however, declared, with full deference to the scientific witnesses, that unless he could find plain evidence that the dermatitis was caused by something other than an irritant substance in the suit he was driven to the conclusion that an irritant must have been there. No such evidence was given, nor was it suggested that any irritant got on to the suit after it was returned from the cleaners. He therefore felt himself bound to infer that whatever got on to the suit got there during the work of cleaning. The firm, who cleaned two thousand suits a week, could not give any details about the handling of this particular suit, but proved that their methods and materials were the best possible. It was common ground that in the ordinary way no irritant ought to get into a suit of clothes, and could only do so through some negligent act. His Lordship was satisfied that somehow the suit became impregnated with a substance which developed an irritating quality and caused the dermatitis. He therefore awarded the plaintiff £79 damages.

In that case a doctor who gave evidence for the cleaners said that some people had an idiosyncrasy which made them peculiarly sensitive to certain irritant substances, but he did not suggest that the patient had such an idiosyncrasy.

In Griffiths *v.* Peter Conway, Ltd., the decision turned on an abnormal sensitivity in the skin of the patient. She had a coat made to her measure, and the second time she wore it, in September, 1937, a rash broke out on her neck. She brought an action against the sellers of the coat at Bristol Assizes before Mr. Justice Branson. The sellers admitted that she had contracted dermatitis and that she would not have contracted it if she had not worn this coat. The law, as declared by Lord Wright in Grant *v.* Australian Knitting Mills,[1] throws on the purchaser the burden of proving that there was some mischievous chemical in the garment, and also that his skin was normal. Dr. W. J. O'Donovan gave evidence that in February, 1938, the lady's skin was very abnormal: the whites of the eyes and the soft palate were insensitive, the skin of the arm was largely anaesthetic, and she gave a dermatographic reaction to the touch of a finger. On the other hand, patch tests with the material of the coat produced no positive reaction, whereas in December, 1937, the reaction to a similar test had been positive. Tests made by Dr. O'Donovan upon himself and upon members of his family and laboratory assistants were all negative. The judge therefore concluded that when the lady had contracted the dermatitis her skin was not in a normal condition. On the other hand, the firm were able to prove

[1] 1936, A.C. 85; *British Medical Journal*, 1935, **2**, 1237.

that the cloth was made exactly as similar cloth had been made for many years in large quantities, and that it contained no harmful material. His Lordship therefore dismissed the action. The lady appealed, and Mr. J. W. Morris, K.C., argued that Section 14 of the Sale of Goods Act, 1893, makes the seller liable even if the buyer has an idiosyncrasy. (That section provides that if the buyer makes known to the seller the purpose for which the goods are required, so as to rely on his skill and judgment, and the goods are of a kind which it is within the seller's business to supply, then there is an implied condition that the goods shall be reasonably fit for that purpose.) The Court of Appeal, however, thought otherwise. Sir Wilfrid Greene, Master of the Rolls, remarked that the particular purpose for which this coat was required was that of being worn by a woman suffering from an abnormality. If such a person desires to obtain the benefit of the implied condition, he said, it is not enough to say that the article of clothing is for her own wear; she must disclose the particular abnormality or idiosyncrasy from which she suffers. He agreed that no one could draw an exact line between normality and abnormality, but pointed out that a judge or jury can easily draw the line without difficulty in any particular case. The lady therefore failed to recover damages.

Universities and Colleges

UNIVERSITY OF DUBLIN

SCHOOL OF PHYSIC, TRINITY COLLEGE

The following candidates have been approved at the examinations indicated:

M.D.—H. Nelson, D. M. Mitchell.

FINAL MEDICAL EXAMINATION.—*Part I* (*Materia Medica and Therapeutics; Pathology and Bacteriology*): †Margaret L. Campbell, †N. M. Smith, †W. Houston, W. F. Rogers, M. Polansky, L. T. Kelly, H. F. Kamp, J. M. Slattery, Yvonne O. Mallet, G. W. E. Little, J. I. Stafford, H. Menter, I. T. Wolpe, Edna G. Merrick, E. P. Hill, C. D. MacCarthy, Olga Bloom, R. W. Carey, H. Hutchinson. *Part II, M.B.:* *M. O'C. Drury, †R. S. W. Baker, †F. de B. Whyte, E. G. Hobart, J. P. Gore-Grimes, E. N. O. Sodeinde, Patricia D. Concannon, M. E. Tapissier, C. J. Du Preez, F. P. O'Sullivan, I. Shreider, H. J. F. Draffin, Lily H. Bamber, Sheila Kenny, E. Cullen, B. Serebro, D. St. B. F. Nunan. *B. Ch.:* †M. O'C. Drury, †M. E. Tapissier, †E. R. F. Mellon, †J. W. Cathcart, †F. R. T. Hollins, L. S. Levinson, S. Sevitt, G. S. Sheill, Mary A. Conyngham, R. A. Lutton, R. C. H. Cooke, L. I. Warnock, T. W. Hanna, D. G. Harrison, Frances M. C. Wolfe, Rosaleen de C. McCormick, E. R. Coetzee, Sylvia M. FitzGerald, Bridget P. Mansfield, E. N. O. Sodeinde, V. T. Williams, N. McSharry, R. J. Sandys, W. Sandford, L. A. S. Edmondson, C. P. Du Preez, N. Marks, J. Freedman, C. H. Rutherford, B. Serebro. *B.A.O.:* *J. McD. Dougan, *P. M. Goedvolk, *C. B. Robinson, †H. D. Chomse, †A. H. Isaacson, †F. L. Willington, †Kathleen Dowling, †D. Selman, †F. de B. Whyte, †Sylvia M. Hall, †J. C. G. Moore, †L. E. Morris, †J. D. Whiteside, G. F. N. Anderson, Muriel Bannister, R. F. G. Lyons, M. Shreider, T. N. Arthur, W. E. Bamford, F. N. C. Levy, W. F. Carpenter, J. K. Craig, B. S. Good, D. W. McCarthy, J. R. Holmes, A. C. Corbett, M. Daly, J. Freedman, A. Graham, G. O. Atkinson, N. C. Brown, Annie Dowds, Lucia M. E. FitzGerald, R. G. P. Heard, G. E. P. Kelly, C. W. Bradfield, N. J. Hogan, R. R. Hogg, J. Morris, S. Tennenbaum, M. H. Waters, W. B. Dennehy, F. A. Hanna, J. H. Tyrrell, Olive S. H. Devlin, Maureen C. Johnston, R. C. H. Cooke, L. A. S. Edmondson, Bridget P. Mansfield, N. McSharry.

DIPLOMA IN GYNAECOLOGY AND OBSTETRICS.—P. C. Das, S. B. Ray.

DIPLOMA IN PUBLIC HEALTH.—*Part I:* †J. C. Gaffney, J. G. Cunningham.

* With first-class honours. † With second-class honours.

ROYAL COLLEGE OF PHYSICIANS OF LONDON

At a meeting of the Royal College of Physicians of London held on April 3 Dr. Robert Hutchison was re-elected President.

Diplomas in Child Health were granted, jointly with the Royal College of Surgeons of England, to the following candidates:

A. Ali, G. C. Babington, S. P. Bellmaine, N. S. Clark, M. S. M. Colswan, W. Freeborn, Frances M. Glyn-Hughes, A. A. Hakeem, Margaret M. Haslam, N. S. Hassan, J. D. Hay, W. Heng, Luk-Yip Hui, Marjorie F. Landau, Sheila M. L. McWilliam, C. N. Mills, Mary E. Nevin, J. L. Parnell, S. M. Paw, Hilda Roberts, A. Rothwell, D. K. Shah, K. C. Shah, J. E. Simpson, J. J. Theron, Margaret I. Williams.

Diplomas in Medical Radiology and Ophthalmic Medicine and Surgery were granted jointly with the Royal College of Surgeons of England to those candidates whose names appeared in the *Journal* of February 18 (p. 365) and March 18 (p. 593) respectively.

ROYAL COLLEGE OF OBSTETRICIANS AND GYNAECOLOGISTS

At the quarterly meeting of the Council held at the Royal College of Surgeons of Edinburgh on April 4, with the President, Professor W. Fletcher Shaw, in the chair, the following were promoted to the Fellowship and formally admitted by the President:

G. S. Davidson, Aberdeen, Alison M. Hunter, Glasgow, T. N. A. Jeffcoate, Liverpool, K. McMillan, Birmingham, Ellen D. Morton, Glasgow, W. C. W. Nixon, London, Elaine M. K. Salmond, London, *W. P. Tew, Canada, Beatrice Turner, London, C. H. Walsh, Liverpool, J. E. R. Heppolette, India.

The following were admitted to the Membership:

P. C. Barkla, Edinburgh, S. G. Clayton, London, A. G. Cumming, New Zealand, J. C. Cuthbert, Sunderland, D. A. Davies, London, J. G. Gallagher, Dublin, *J. E. Giesen, New Zealand, *R. E. Hirson, Manchester, *F. D. Johnston, Canada, O. V. Jones, Bangor, N. K. B. Kimbell, London, A. W. Lawler, Canada, A. W. Louw, South Africa, *W. K. McIntyre, Tasmania, Mary H. Mayeur, London, G. P. Milne, Aberdeen, *A. J. Murray, Australia, E. L. Nicolson, Sheffield, Sophie Schiller, South Africa, J. Smibert, Australia, J. K. Sutherland, London, Eileen M. Whapham, London, Margaret M. White, London.

* In absentia.

J. L. Cameron, London, was elected to the Membership.

At the annual general meeting of the College, held at the Royal College of Surgeons of Edinburgh on April 4, with the President, Professor W. Fletcher Shaw, in the chair, the following were elected to Council in place of those retiring by statutory rotation. *Representatives of the Fellows:* Mr. William Gilliatt, Mrs. Hilda Lloyd, Mr. W. F. T. Haultain, Professor C. G. Lowry. *Representatives of the Members:* Mr. A. C. H. Bell, Mr. John Sturrock.

The Services

ARMY MEDICAL ADVISORY BOARD

The War Office announces that J. R. Learmonth, Ch.M.Glas., F.R.C.S.Ed., Professor of Surgery in the University of Edinburgh, has been appointed a member of the Army Medical Advisory Board in succession to the late Sir David Wilkie.

AUXILIARY R.A.M.C. FUNDS

The annual meeting of the members of the Auxiliary R.A.M.C. Funds will be held at 5.15 p.m. on Friday, April 28, at 11, Chandos Street, Cavendish Square, W., when the annual report and financial statement for the year ended December 31, 1938, will be presented and the officers and committee for the current year elected.

DEATHS IN THE SERVICES

Lieutenant-Colonel ANDREW BUCHANAN, Bengal Medical Service (ret.), of St. Jacques, Guernsey, and Killyclogher, Omagh, died on March 26, aged 77. He was born on May 17, 1861, the son of John Buchanan of Omagh, and was educated at Queen's College, Belfast, and graduated at the Royal University of Ireland as B.A., with honours and gold medal, in 1882, proceeded M.A. in the same year, received the M.D. in 1885, and the M.Ch. in 1886. He also studied for a time at St. Bartholomew's Hospital. He entered the Indian Medical Service as surgeon on March 31, 1887, became lieutenant-colonel after twenty years' service, and retired on March 20, 1921. He served on the North-East Frontier of India in the Chin-Lushai Campaign of 1889–90, receiving the frontier medal with a clasp. Most of his service was spent in civil employ in the Central Provinces, where he was for some years civil surgeon of Saugor, and later of Nagpur. He was the author of *Diseases of the Gums in Indian Jails*, 1889; *Malarial Fevers and Malarial Parasites in India*, 1901; *Lathyrism in the Central Provinces*, 1904; and *Plague Prevention in Nagpur, Cats as Plague Preventers*, 1909. He had been a member of the British Medical Association for forty-eight years, and was chairman of the Guernsey and Alderney Division in 1930–1 and president of the Southern Branch in 1933–4.

Medical News

The King, accompanied by the Queen, will open the new Westminster Hospital on Thursday, April 20, at 3 p.m.

The one hundred and third annual general meeting of the Royal Medical Benevolent Fund will be held in the Lecture Room of the Medical Society of London, 11, Chandos Street, Cavendish Square, W., on Tuesday, April 25, at 5 p.m., with the president, Sir Thomas Barlow, Bt., in the chair.

The Hunterian Lecture will be delivered before the Hunterian Society at the Mansion House, E.C., on Monday, April 17, at 9 p.m., by Professor Emil de Grósz of Budapest on "The Problem of Glaucoma."

At a public luncheon to be held on Friday, April 28, at Prince's Restaurant, Piccadilly, W.1, Dr. Elton Mayo, Professor of Industrial Research at Harvard University, will speak on "Some New Problems of Administration." The luncheon is organized by the National Institute of Industrial Psychology, Aldwych House, Aldwych, W.C.2. A demonstration of psychological methods of vocational guidance will be held at 3 p.m. at the Institute following the luncheon, and guests are cordially invited. By reason of limitation of space, admission will be by ticket.

The Paris Faculty of Medicine, in co-operation with the Association for Development of Medical Relations and the American Medical Society of Paris, has organized a series of courses in English opening in the second fortnight of June. Further information can be obtained from A.D.R.M., Salle Béclard, Faculté de Médecine, 12, Rue de l'École de Médecine, Paris, 6e.

The Nutrition Panel of the Food Group of the Society of Chemical Industry has arranged a conference on "Vitamin E," to be held at the London School of Hygiene and Tropical Medicine, Keppel Street, Gower Street, London, W.C., on Saturday, April 22. At the first session, from 10.45 a.m. to 12.45 p.m., the chemistry of vitamin E will be discussed by Professor P. Karrer of the University of Zurich, Dr. A. Emmerie of the University of Utrecht, and Dr. E. Lester Smith. At the second session, from 2.15 p.m. to 4.15 p.m., the physiological action of vitamin E will be considered by Dr. Karl E. Mason of the Vanderbilt University, Tennessee, Dr. S. W. F. Underhill, Dr. T. Moore, Miss A. M. Copping, Professor V. Korenchevsky, Dr. B. P. Wiesner, and Mr. A. L. Bacharach. At the third session, from 5 p.m. to 7 p.m., the clinical and veterinary uses of wheat-germ oil and vitamin E preparations will be discussed by Dr. E. Vogt-Möller of Copenhagen, Dr. E. Shute of the University of Western Ontario, Mr. D. W. Currie, Dr. G. C. M. M'Gonigle, and Mr. C. R. Macdonald. The three sessions will be under the chairmanship of Professor A. R. Todd of the University of Manchester, Professor J. C. Drummond of University College, London, and Professor F. J. Browne of University College Hospital, London, respectively. No ticket is required for admission.

The annual congress of the Ophthalmological Society of the United Kingdom will be held at 1, Wimpole Street, W., on Thursday, Friday, and Saturday, April 20, 21, and 22, under the presidency of Mr. T. Harrison Butler, whose opening address will deal with the anterior capsule. Professor Weve's Bowman Lecture is entitled "Diathermy in Ophthalmic Practice," and the subject for the main discussion on April 21 is "The Problems of Refraction." A large number of papers will be read during the congress, and a trade exhibition has been arranged in the adjoining house of the College of Nursing. The International Association for the Prevention of Blindness and the International Organization against Trachoma are holding their annual meetings during the period of the congress; the former will discuss the application of the Crédé method for the prevention of ophthalmia neonatorum in various countries.

The nineteenth Congress of German Oto-rhino-laryngologists will be held at Vienna from May 25 to 27 when the chief subject will be the control of lupus in dermatology and oto-rhino-laryngology.

On the death of Mr. J. A. Lyons, on April 7, Sir Earle Page, M.B., Minister of Commerce and Deputy Premier, was sworn in as Prime Minister of Australia. According to present arrangements Sir Earle Page will resign his new post after Parliament meets on April 19 in favour of a leader from the United Australia Party.

Five hundred delegates from gas undertakings throughout the country will attend the annual conference of the British Commercial Gas Association, to be held in Brighton from April 22 to 25. Among the speakers will be Lord Horder and Sir Harold Hartley, chairman of the Fuel Research Board.

EPIDEMIOLOGICAL NOTES

Influenza

The week has seen a rise in the incidence of pneumonia (primary and influenzal) for England and Wales as a whole, but it is still less than in the corresponding week of last year and much less than the median value for the last nine years. In Scotland, Eire, and Northern Ireland small decreases were reported, and the figure for London was 118 for both weeks. Deaths from influenza showed a considerable reduction for the 126 great towns—152, as against 174—in which London shared—32, compared with 49. Figures for other towns were: 12 in Birmingham, 5 each in Cardiff, Portsmouth, and South Shields, 4 each in Bath, Bristol, Hendon, Liverpool, and Manchester.

The data on the incidence of influenza collected by the Statistical Supplement to the *Weekly Epidemiological Record* of the Health Section of the Secretariat of the League of Nations show well the spread of influenza in the early weeks of 1939. In the four-week period ended January 28, 12,765 cases were recorded in the U.S.A., compared with 7,736 in the immediately preceding four-week period. In Denmark 16,388 cases were notified compared with 5,595, in Norway 6,344 compared with 3,042, and in Finland 3,514 compared with 2,435. The figures for influenza in the three countries from which the data are available show that influenza spread considerably in the next four-week period, the numbers being: Denmark 38,799, Switzerland 37,231 (compared with 11,520), and Finland 5,031.

Diphtheria

Notifications of diphtheria in England and Wales showed a small rise, the first since the week ended January 28. The progressive fall during these two months affected the whole country, both urban and rural districts. South Shields has remained a notable exception since the beginning of 1938, and despite an intensive immunization campaign, first for school children and later extended to pre-school children, both incidence and deaths remain high. During 1938 there were 589 cases and 62 deaths, and during the first three months of 1939 225 cases with 20 deaths. It is probable that in recent months the number of cases notified has been unduly high owing to the inclusion of carriers, but the high case fatality indicates that the disease is still prevalent in the community and in a virulent form.

Infant Mortality in Certain Great Towns

The infant mortality rates for great towns for the four-week period ended January 28, 1939, and the median values based on the corresponding periods of a varying number of years immediately preceding (figures shown in parentheses) are given in the *Weekly Epidemiological Record* of the League of Nations. This figure is generally taken as being one of the most reliable criteria of the health and social conditions of the community. Towns with rates over 100 (per 1,000 live births) are: Belfast 184 (143), Dublin 148 (96), Glasgow 107 (125), Warsaw 151 (113), Lille 119 (92), Liége 116 (84), Cologne 105 (92), Königsberg 103 (84), Bangkok 133 (193), Singapore 163 (201). Low rates were recorded in Washington (D.C.) 31 (49), Amsterdam 41 (42), Dresden 34 (70), Oslo 32 (45), Stockholm 21 (42), Sydney 37 (40).

No. 13

INFECTIOUS DISEASES AND VITAL STATISTICS

We print below a summary of Infectious Diseases and Vital Statistics in the British Isles during the week ended April 1, 1939.

Figures of Principal Notifiable Diseases for the week and those for the corresponding week last year, for : (a) England and Wales (London included). (b) London (administrative county). (c) Scotland. (d) Eire. (e) Northern Ireland. Median values for the last 9 years for (a) and (b).

Figures of Births and Deaths, and of Deaths recorded under each infectious disease, are for : (a) The 126 great towns (125 in 1938) in England and Wales (including London). (b) London (administrative county). (c) The 16 principal towns in Scotland. (d) The 13 principal towns in Eire. (e) The 10 principal towns in Northern Ireland.

A dash — denotes no cases ; a blank space denotes disease not notifiable or no return available.

Disease	1939					1938 (Corresponding Week)					1930–8 (Median Value Corresponding Weeks)	
	(a)	(b)	(c)	(d)	(e)	(a)	(b)	(c)	(d)	(e)	(a)	(b)
Cerebrospinal fever	38	6	5	3	—	32	6	12	3	—		
Deaths		—	—				1	4				
Diphtheria	922	88	224	54	18	1,388	144	239	76	38	1,099	150
Deaths	22	—	6	2	—	25	5	6	3	1		
Dysentery	32	5	36		—	111	21	54		—		
Deaths				—	—			—	—	—		
Encephalitis lethargica, acute	2	—	—		—	13	—	—		—		
Deaths		1					1	1				
Enteric (typhoid and paratyphoid) fever	12	1	6	—	—	8	1	6	2	3	19	
Deaths	—	—	—	—	—	—	—	1	—	—		
Erysipelas			61	3	2			70	3	5		
Deaths		—					3	—				
Infective enteritis or diarrhoea under 2 years												
Deaths	50	14	8	3	1	33	14	9	7	3		
Measles		31	184		7			1,421		63		
Deaths	5	—	—	—	—	38	12	19	2	8		
Ophthalmia neonatorum	100	14	38		1	105	13	31	1	—		
Deaths												
Pneumonia, influenzal*	1,175	118	16	7	11	1,205	120	14	5	23	1,367	129
Deaths (from influenza)	153	32	5	1	1	74	13	5	1	1		
Pneumonia, primary			163	10				211	12			
Deaths		2		13	9		29		21	13		
Polio-encephalitis, acute	—	—				3	—					
Deaths		—					—					
Poliomyelitis, acute	2	—	1		—	4	—	—		—		
Deaths		—					—	—				
Puerperal fever	5†	5	12		—	8†	8	21	2	1		
Deaths		—					3‡					
Puerperal pyrexia	152	11	21		4	190	25	33		1		
Deaths												
Relapsing fever	—	—			—	—	—			—		
Deaths												
Scarlet fever	1,675	152	248	50	66	2,323	188	367	88	69	2,263	237
Deaths	1	—	1	—	—	5	—	6	—	1		
Small-pox	—	—	—		—	—	—	—		—		
Deaths	—	—		—	—	—	—	—	—	—		
Typhus fever	—	—	—	—	—	—	—	—	—	—		
Deaths				—	—			—	—	—		
Whooping-cough		322	502		47			85		12		
Deaths	16	2	10	3	2	28	4	4	1	1		
Deaths (0–1 year)	398	59	69	25	18	357	64	78	52	24		
Infant mortality rate (per 1,000 live births)	62	50				59	52					
Deaths (excluding stillbirths)	5,770	1,180	637	207	128	4,720	916	698	239	155		
Annual death rate (per 1,000 persons living)	14.2	15.0	12.9	13.9	11.2	11.6	11.5	14.2	16.1	13.7		
Live births	5,957	1,039	983	393	225	6,739	1,323	934	392	216		
Annual rate per 1,000 persons living	14.6	13.2	19.9	26.4	19.8	16.6	16.7	19.1	26.5	19.1		
Stillbirths	266	43	48			290	42					
Rate per 1,000 total births (including stillborn)	43	40				41	31					

* Includes primary form in figures for England and Wales, London (administrative county), and Northern Ireland.

† After October 1, 1937, puerperal fever was made notifiable only in the administrative county of London.

‡ Deaths from puerperal sepsis.

Letters, Notes, and Answers

All communications in regard to editorial business should be addressed to THE EDITOR, BRITISH MEDICAL JOURNAL, B.M.A. HOUSE, TAVISTOCK SQUARE, W.C.1.

ORIGINAL ARTICLES and LETTERS forwarded for publication are understood to be offered to the *British Medical Journal* alone, unless the contrary be stated. Correspondents who wish notice to be taken of their communications should authenticate them with their names, not necessarily for publication.

Authors desiring REPRINTS of their articles published in the *British Medical Journal* must communicate with the Secretary, B.M.A. House, Tavistock Square, W.C.1, on receipt of proofs. Authors over-seas should indicate on MSS. if reprints are required, as proofs are not sent abroad.

ADVERTISEMENTS.—All communications should be addressed to the Advertisement Manager (office hours 9 a.m. to 5 p.m.). Orders for copies of the *Journal* and communications with reference to subscriptions should be addressed to the Secretary, B.M.A. House.

The TELEPHONE NUMBER of the British Medical Association and the *British Medical Journal* is EUSTON 2111.

The TELEGRAPHIC ADDRESSES are

EDITOR OF THE *BRITISH MEDICAL JOURNAL, Aitiology Westcent, London.*

SECRETARY, *Medisecra Westcent, London.*

The address of the B.M.A. Scottish Office is 7, Drumsheugh Gardens, Edinburgh (telegrams: *Associate, Edinburgh;* telephone 24361 Edinburgh), and of the office of the Cumann Doctúirí na h-Éireann (I.M.A. and B.M.A.), 18, Kildare Street, Dublin (telegrams: *Bacillus, Dublin;* telephone 62550 Dublin).

QUERIES AND ANSWERS

Thumb-sucking

"A. N. F." writes: I should be glad if someone can give me advice on thumb-sucking in a girl aged 2; the habit has persisted since soon after birth, and it is confined to the left thumb. No really active measures have been taken yet.

Snoring

Mr. EDWARD DENGROVE (Edinburgh) writes: Concerning "Perplexed" and his case of snoring (March 25, p. 655), may I be allowed to suggest that perhaps the complaint is only an outward manifestation of an underlying dissatisfaction. Many men snore, but few wives show the response that "Perplexed's" patient has shown. Note that the patient is married only three months, that she does not sleep with her husband under the excuse that he snores, and that a separation is even being considered for the same reason. Could it not be that this woman is dissatisfied with her husband, married life, or both? Is it not possible that a marital incompatibility has arisen, perhaps as regards sexual satisfaction?

"T. G. N. S." writes: The following experience may perhaps be of use. After an abdominal operation a patient developed the habit of sleeping on his back; snoring followed with the usual inconvenience. It was found that a narrow strip of adhesive plaster applied perpendicularly down the ridge of the nose to a point under the chin anchored the lower jaw so that the mouth remained shut; complete cessation of snoring followed. The strip of plaster can be applied in the dark and can be released and reapplied at any time if desired.

Treatment of Urticaria

Dr. COLLIN N. JANOVER (Franklin, E.G., South Africa) writes: In reply to "M.B." (February 25, p. 430), who has a case of urticaria that does not respond to the usual treatment, I advise him to try anti-syphilitic treatment—that is, N.A.B. intravenously, spirobismol intramuscularly, pot. iod. by the mouth, and ung. hydrarg. to the rash even if the Wassermann reaction is negative. I have had similar cases, not only with "urticaria" but with other skin conditions, in which prolonged treatment with the usually recognized methods failed, and where anti-syphilitic treatment, even if the Wassermann reaction was negative, has given excellent results. I may mention that the pot. iod. is combined with mag. sulph. and liq. hydrarg. perchlor.

A Result of Circumcision

Dr. M. FISHER (Oxford) writes: I should like to make some comments on the query of "H. C. M." (March 18, p. 597). It seems to me that the conclusion to be drawn from the correspondence arising from Mr. Kenneth Walker's article on circumcision (December 31, 1938, p. 1377) is that in infancy and early childhood adhesions between prepuce and glans are normal and almost universal, while in later childhood and adolescence adhesions are absent. It therefore follows that a natural breaking down occurs at some point in the child's life. This is surely what is now happening in "H. C. M.'s" patient—his previous incomplete circumcision having no bearing on the question. It seems unlikely that this process, which must be regarded as a natural one, has any relation to the enuresis.

LETTERS, NOTES, ETC.

Suggested Technique for Nursing Incontinent Patients

Dr. G. G. W. HAY (County Hospital, Whiston) writes: The use of the absorbent properties of sphagnum moss for taking up discharges is well known, and was utilized to a considerable extent during the last war. This property suggests an extended use in absorbing the discharges of the incontinent patient—either adult or child. The usual method of using this moss as proposed in some textbooks seems not only clumsy but most unsatisfactory, because it would be in continuous contact with the buttocks and genitals and when moist would act more or less as a poultice. The nursing of incontinent patients has been a problem I have had to cope with for many years, and although the usual accepted methods have much to commend them I have now come in contact with a method of using sphagnum moss along with certain allied material which appears to be very efficient. The method of using the moss is rather unusual, but in that lies its advantage. For approximately three months cases with incontinence and bed-sores have been handled on this particular mattress with encouraging results. Cases in which urine rashes refused to respond to treatment with methylated spirit and soap and water, or even to castor oil, have responded to the moss mattress. It ought to be emphasized that the usual skin toilet carried out during the changing of the patient was omitted. A mattress made up of interchangeable sections containing the moss, etc., was used, and it was noticeable that the sections, although actually absorbing fluid, were never wet on the surface, and also that there was no offensive odour. Any section actually showing moisture was exchanged for a new one. These sections can be sterilized by heat, and this process drives off all ammoniacal products and renders the sections ready for further use. Senile incontinent cases were deliberately allowed to go for twenty-four hours without changing, and at the end of this time they were still satisfactory; the mattress was only slightly damp, while the odour was much less than had previously been the case with four-hourly changing. There appears to be a definite field for such a mattress, which the designer and patentee desires to be known as the Sandry mattress. Commercial production has not yet begun, but supplies will be sent to medical superintendents who wish to test the possibilities on application to Mr. Myer Blumfield, 149, Alder Road, Liverpool, 12, or Messrs. White and Wright, 93, Renshaw Street, Liverpool, 1.

Charterhouse Rheumatism Clinic: A Correction

Dr. H. WARREN CROWE (London) writes: May I be permitted to correct an error in your report of my speech (*Journal*, April 8, p. 745) at the opening of the Charterhouse Rheumatism Clinic. You quote me as having said that the clinic had been started with the idea that serious forms of rheumatism and arthritis should be treated at special centres. My actual words were as follows: "I think it is true to say that all plans which have been put forward to deal with the more serious forms of rheumatism and arthritis have been based on the assumption that cases must be treated at special centres. The supply of such centres is totally inadequate. This at the moment presents an insurmountable difficulty. It would, however, be greatly mitigated if treatment could be administered in the home. The Charterhouse Rheumatism Clinic was started with the idea of finding a method of treatment whereby this object could be attained. . . . I can say that a very considerable success has been achieved in what one might reasonably call the home treatment of rheumatism." So that your report inadvertently gave exactly the contrary impression. Further, I made it quite clear that investigations at a special centre were required in only about half the total number of cases. The point I was trying to bring out was that the methods of the Charterhouse Clinic were adaptable for the use of the general practitioner *without* the necessity for treatment at a special centre.